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Part First.

ORIGINAL COMMUNICATIONS.

I.—VALEDICTORY ADDRESS TO THE EDINBURGH MEDICO-CHIRURGICAL SOCIETY.

By the Retiring President, HENRY DUNCAN LITTLEJOHN, M.D., F.R.C.S. Ed.

(*Delivered 4th November 1885.*)

IN selecting a topic for this valedictory address I naturally fall back on those departments of medicine with which I am most familiar, and in which I have worked during my professional life. I trust, therefore, the members of this Society will bear with me for a short time as I treat on some topics in medical jurisprudence and public health which have specially attracted my attention.

First, As to the teaching of Medical Jurisprudence. It has often struck me with surprise that so few of the many lecturers on this important subject fill the post of surgeon of police, and thereby enable themselves to acquire a *practical* acquaintance with the many forms of violent death, not to speak of the opportunities afforded by that position of giving evidence in courts of law, and of becoming familiar with medico-legal questions of all kinds. It is not to be wondered at that medical jurisprudence is not regarded with interest by the student while preparing to undergo his final trials, and that when he settles down in practice he, as a rule, eschews all cases which may necessitate his appearance in the witness-box. This indifference on the part of the student and practitioner helps to account for the general unsatisfactory nature of medical testimony on trials, and for the continuance of that distrust by the legal profession of medical opinion in a court of law.

The present system of teaching is, in my opinion, fundamentally wrong. The lecturer should be practically conversant with the subject matter of his lectures, and his prelections, instead of consisting of a digest of some of the more popular text-books, should be based on his experience, and, in addition, the student should be brought face to face with the facts of the science in the presence of his teacher, who ought to have the opportunity of demonstrating

them on the dead or living body. It is only thus that an intelligent interest can be excited in the student, who is thus led to study the subject with practical zest. Facts, the knowledge of which he previously acquired in the course of his reading, now assume an importance and a definition which fix them on his memory in a manner which mere reading can never do. And what is generally regarded by the student as one of the most unsatisfactory subjects of his medical studies becomes at once full of interest, and the lecture-room, instead of being a species of dreary prison-house, is converted into a place where the intelligent student feels he is truly gaining knowledge, and that of a kind which is certain to be of use to him in all departments of his profession.

Where such a method of practical instruction is not available, the student for his examinations has to trust to his notes or to some text-book. Such a method of cramming accounts for the unsatisfactory nature of the examinations on this important subject of medical study. There is nothing more disheartening for an examiner than to have to question a candidate, for example, on the subject of Drowning, and to find that he has never had an opportunity afforded him of seeing a body which has been recovered from the water, and of having pointed out to him by his teacher the various changes incident to immersion in water. It is like examining a student on "Delivery of the Pregnant Female" who has never attended a case of confinement, and who trusts merely to his lecture-notes to enable him to answer the questions of his examiners. How useless it would be to question a student on the subject of Chest Disease who had never walked the wards of a hospital, and watched cases for himself under the eye of his teacher!

What I complain of is that, while due care has been taken to supply the other subjects of medical study with their practical departments, so to speak, no endeavour has been made by the Medical Council and the various licensing bodies to insist that medical jurisprudence should be similarly treated. Thus anatomy has its dissecting rooms; chemistry, physiology, and pathology their laboratories; medicine and surgery and obstetrics, fully equipped hospitals and dispensaries—in all of which the student obtains practical instruction.

Unfortunately, owing to this want and other causes, medical jurisprudence in all our schools is comparatively neglected. Take any of the provincial or even metropolitan schools—medical jurisprudence is regarded with such indifference that it is generally selected as the subject supposed to be most easily taught, and it is usually assigned to the youngest and least experienced member of the teaching staff, although, as you all know, the subject matter of his lectures is drawn from every department of medical study. A case of poisoning, for example, can only be fully investigated by means of very varied knowledge. The *symptoms* must be contrasted

with those of some disease suddenly established, constituting what is called the "differential diagnosis," by which the medical attendant, in circumstances of great difficulty, is enabled not only to refer the symptoms to some one class of poisons, but even possibly to one special poison, thereby enabling him to select at once the appropriate antidote, and thus save life. Should he be unsuccessful in this, and a *post-mortem* examination is ordered, he must be able to recognise and describe the appearances in the dead body with the skill of a pathologist; and, lastly, he must aid the authorities in the further investigation of the case by removing those organs and fluids in which the chemist may most readily find traces of the poison.

Our universities are not much better off. With one or two brilliant exceptions, the occupants of the chairs of Forensic Medicine are never called upon to give evidence in a court of law or to conduct medico-legal inquiries. They have no opportunity of bringing under the notice of their classes those cases of sudden death, suicide, etc., etc., which, as you will observe, are of too common occurrence in all our towns. The nature of their prelections can easily be imagined, and with this, which I hold to be, imperfect knowledge, the student passes his final trials and enters on practice. The very first case which comes under his notice may demand from him some medico-legal knowledge and acquaintance with official forms with which he is totally unacquainted. The authorities are put to much inconvenience in consequence of this ignorance; and, in my experience, the facts of the case have too often been so imperfectly noted and reported that the ends of justice have been frustrated.

Now, the remedy I would suggest is this, that medical jurisprudence should be practically and intelligently taught, and this I hold can only be done by the lecturer having some official relation to our criminal authorities. In all our medical schools endeavours should be made to combine the teaching of the subject with the duties of such an office as the surgeoncy of police. It is the official who occupies that position that is summoned to see all cases demanding the cognizance of the police; and as he is generally the adviser of the criminal authorities, he is naturally called upon to make dissections and to conduct inquiries of all kinds relating to the subject-matter of his course of lectures; and he is thus, as I have said, enabled to preserve that *practical* tone in his prelections which is, in my opinion, absolutely necessary to give them any authority, and at the same time (a matter of some moment) to interest his jaded auditory; for we must remember that the medical student of to-day is over-lectured, and unless we can appeal practically to more senses than the ear, our tuition becomes comparatively valueless.

When I turn, *Secondly*, to *Medico-legal Practice*, I approach a delicate topic, involving the relations between the criminal autho-

rities and the profession. And yet I hold that, if we follow the guiding of the "Golden Rule," it is impossible that these relations can ever lead to misunderstanding and breaches of medical etiquette. The authorities have an undoubted right to avail themselves of the best skill and the most mature experience they can command. But the medical man selected ought to remember that in discharging the special duty to which he has been summoned, he has also a professional duty to discharge. Nothing can be more galling for a practitioner than to find himself and his evidence disregarded in the investigation of an important criminal case, when he is conscious that he was the first summoned, and that thus he had the opportunity of observing facts of great importance. The ready answer of the public prosecutor is that he alone is responsible to the Crown for the proper investigation of crime, and that in the exercise of this responsibility he is bound, with a due regard to the public interest, to summon to his aid the best skill of the town or country. This is so far true; but as two medical men are required in an important case in our courts of law, I hold that it is essential that one of these should be the medical man who was first summoned, and who, in all probability, has been put to much inconvenience by being called by the police or the neighbours from other duties, and at an untimely hour. But let the action of the authorities be what it may, as it is often difficult to get a layman, be he a lawyer or not, to sympathize with the unwritten law of medical etiquette, the duty of medical inspectors is plain,—it is at least courteously to afford the medical man who first saw the case an opportunity of being present at the dissection, and thereby to recognise his position and to lessen his mortification at being overlooked by the authorities. A circumstance has comparatively recently occurred which renders the caution I have just stated of increased importance.

It was for a long period made the subject of repeated complaint, that while in England the medical practitioner was paid the usual honorarium for the dissection, and also for his subsequent evidence at the inquest, in Scotland only one fee was paid for the dissection and for the written medical report, which is the equivalent to the *vivâ voce* testimony at the English inquest. Lord Advocate Watson, now Lord Watson, has the merit of recognising this discrepancy, and of influencing the Government of the day to raise the fee of a dissection and a medical report from £1, 1s. to £2, 2s. This was a tardy act of justice, but unfortunately it has been clogged with certain formalities which have prevented it being the boon it was expected to be to our profession. Every such increased fee must be made the subject of a separate account, which is made out by the local procurator-fiscal, sent to the Crown office, and there receives the official signature of the Lord Advocate or Solicitor-General. All this involves trouble to a set of already hard-worked, and in many cases underpaid, highly responsible

officials. The result may easily be imagined. A dissection is now ordered with a certain amount of difficulty and increased caution; and what is, in my opinion, more detrimental to the profession and to the public interest, in order apparently to lessen the amount of the increased expense of the augmented fee to the country, the dissection is now, for the first time, generally intrusted to a single medical man. Now, with all my experience, extending over a long series of years, I must protest against this copying of the reprehensible practice which too often obtains in England, viz., that in important medico-legal investigations respecting serious and mortal crimes, the testimony of only one medical witness is produced on the trial. Not only is such a course fitted to imperil the prosecution of crime, but it also adds greatly to the difficulties of the prisoner. Is it fair, is it proper, that a prisoner's life should depend on the unsupported evidence of a single professional witness, however eminent that witness may be? If the law of commonsense demands that all important facts in the preliminary investigation of crime should be spoken to by two witnesses, it is certainly equally important that those facts connected with the post-mortem examination should be satisfactorily proved by the evidence of two trustworthy medical men. I certainly shrink from the sole conduct of these delicate and important investigations; and I have to reiterate, which I have often stated, that the most experienced expert has the greatest personal satisfaction and comfort when associated with another medical man, who, although possibly comparatively inexperienced, fulfils the important function of corroborating his testimony and of speaking conclusively to the existence of possibly crucial injuries, which may prove the guilt of the prisoner at the bar, or else free him from a capital charge.

It is the duty of the profession—certainly it is, in my opinion, of this Society—respectfully to remonstrate with the action of the criminal authorities in this matter, and to urge upon them the necessity of employing two medical men in the investigation of all important crimes involving the life or honour of a fellow-citizen.

I now turn shortly to the discussion of one or two topics connected with *Public Health*.

In the *first* place, I hold it to be one of the principal duties of the health-officer to furnish to the public, through the medium of the press, a weekly statement of the health of the community to which he is attached. It is a matter of never-ending surprise to me that the great metropolis and other centres of population should not be provided with such a statement. Somewhere about Wednesday in the week a statement is furnished; but this is generally limited to a simple notice of the rate of mortality. This conveys little or no information that can interest the public or the profession; and I have always held that it is desirable that more facts should be supplied. For more than twenty years the citizens

of Edinburgh have been furnished on Monday mornings not merely with the total mortality of the preceding week, and the calculated death-rate, but also with the respective mortalities of the chief divisions of the city, the inhabitants being thus supplied with important information as to the health of the districts in which they live. Why cannot similar information be afforded to the citizens of London and other centres of population? What use can it be to the inhabitants of any of the districts of London to be informed on Wednesday of the total death-rate of a population consisting of four millions? The post-office authorities, in order to facilitate the mere delivery of letters, has subdivided London into numerous districts. Why cannot the Registrar-General for sanitary or statistical purposes not make a less elaborate division, and every Monday morning let the inhabitants of important districts, such as, *e.g.*, Kensington, know the death-rate of the surrounding population? It is a trite remark, that stale information is uninteresting and comparatively valueless; and it is not to be wondered at that citizens, when treated to statistics in many cases a fortnight old, give little heed to them, and are thus led to regard with indifference questions relating to public health. What can be of more importance to a citizen than the sanitary state of his surroundings? These directly influence the death-rate and the causes of death,—a statement of which, therefore, will certainly be regarded by the householder with, at any rate, equal interest to a statement of the weekly variations of stocks. It certainly may be urged on the score of economy, that notwithstanding his army of clerks, it is less the duty of the Registrar-General to furnish this information than the various local health-officers. Were each of these officials to undertake the duty as it is done in Edinburgh, it could easily be accomplished by such division of labour with this attendant good, that the health-officer, in tabulating weekly the causes and the localities of death, gets a grip, so to speak, of his special duties, and observes for himself, and not at second-hand, what areas more especially demand his inspection. But what is the actual state of matters in the metropolis? Elaborate information is annually afforded by each health-officer in a voluminous report which, like similar documents, is too generally regarded as waste paper, and treated accordingly. Lastly, one great advantage of the plan followed in this northern metropolis of distributing the mortality—the *causes of death*, the *ages at death*, and specially the *zymotic mortality*—is, that each citizen is thus enabled to observe for himself the state of prevailing diseases, and to obtain information, should he desire it, as to the steps taken by the authorities to meet any special emergency.

Since November 1879, in addition to the weekly statement of the mortality of the city, the amount of cases of the more important forms of infectious disease have also been tabulated; and

for this we are indebted primarily to the courtesy of the profession, and, secondly, to the enlightened liberality of our Town Council. The following statistics will, I have no doubt, interest my audience. Since November 1879 the total number of intimations of cases of infectious disease sent in by the medical practitioners of Edinburgh and the surrounding districts, up to the present date, amounts to 29,000; and the sum disbursed by the Town Council to the profession for this valuable information amounts to nearly £4000.

In no city in the world has the system of notification had a fairer trial or been more successful. The first result of the system was to open the eyes of the citizens and the profession to the enormous amount of preventible disease that lay simmering in our midst, the existence of which was only too tardily and partially revealed by the mortality returns; and, second, to convince our Town Council of the utter insufficiency of the accommodation provided in Edinburgh for the isolation and treatment of infectious disease. This led to instant action on the part of the authorities; and at an expense of fully £30,000, Edinburgh is now provided with a satisfactory infectious hospital, which will be available as an important adjunct to our Medical School, by affording our students and young practitioners an opportunity of studying, under favourable circumstances, those important diseases,—the treatment of which constitutes, as we all know, such a large part of ordinary medical practice.

Again, it cannot be doubted that this weekly statement of the number of cases of infectious diseases in the three leading divisions of our city must be of interest and value to the medical practitioner. His attention is thus pointedly directed to the leading forms of zymotic disease in our midst and their relative prevalence; and he is thus enabled, in many cases, to make a more rapid and trustworthy diagnosis.

But other and equally important advantages have attended the introduction of the notification of these diseases. In intimating them, as I have been repeatedly informed by my medical brethren, the profession as a body has been reminded of its duty to society in preventing, as far as possible, the spread of the disease by suitable isolation, and by taking every step that science has suggested for removing those conditions that favour its propagation. It is gratifying to observe that year by year the number of calls made on the municipal authorities to convey cases to hospital has increased; and it is to be hoped that, with the ample accommodation now afforded, every suitable case will be isolated in our hospital wards. Equally gratifying results have attended the notification in the marked attention now paid by the profession to the sanitary conditions in which the patients live. I have again and again been told that the mere filling up of the intimation card has led the practitioner to reflect on the surroundings of

his patient, and to call on the authorities to remedy sanitary defects which otherwise might, in the hurry of practice, be overlooked and never remedied, until such a mortality, or such amount of illness, was caused that they imperatively called for summary remedy.

This system of notification has been attended with such marked success, mainly by the cordial assistance afforded by my professional brethren; and also, it must be allowed, in consequence of the recognition, inadequate though it may appear, by the municipality of the value of their services. It was my duty, as the medical adviser of the corporation, to insist upon some recognition, and I am bound to state that my representations were at once given effect to; and so important has the system of notification appeared to the municipality, and so necessary for the maintenance of the reputation of our city as one of the healthiest in the country, that the medical grant has always been most cheerfully paid by the ratepayers, and no grumble has ever been made, even in these, for all classes of society, economizing and unpropitious times. This is a sufficient answer to those of our English and Scotch brethren who, failing to recognise the importance of early intimation of infectious disease, and refusing to avail themselves of our northern experience, obstinately shut their eyes to the advantages of this important measure. All the evils that were prophesied by gentlemen, regarded as leading authorities in sanitary science, when the system was boldly inaugurated in Edinburgh, have by our experience been proved to be groundless; and my presence in the distinguished position of your President and the influential audience I have the honour to address, conclusively prove that intimation of infectious disease is no sham, but an accomplished fact; and that it can be carried out in a large city and in the midst of a powerful Medical School, without the slightest infringement of medical etiquette, and in such a manner as to secure the co-operation of every member of our profession.

Without some such measure it is utterly impossible, in my opinion, to cope successfully with infectious disease. Were notification generally adopted, the country would have been saved the danger and the scandal of a chronic epidemic of smallpox in London, and of this and other diseases in other large towns. But the important lesson we have learnt in Edinburgh must not be forgotten, viz., that along with a system of notification there must be ample means for suitable isolation. Unless these be combined, little effect will be exercised on the spread of these diseases; and the whole history of the present discussion as to vaccination and smallpox goes to show that, in order to stamp out that disease, notification and revaccination are required, and, in addition, the speedy isolation of every suitable case in a well-appointed hospital. The mention of this loathsome disease reminds me that our ex-

perience in Edinburgh of the benefits attending suitable isolation has been strangely overlooked in the interminable discussions, in the southern medical journals, as to the danger incident to small-pox hospitals. Here in 1871 we were visited with a severe outbreak of smallpox, and advantage was taken by the municipal authorities of Watson's Hospital—the well-known educational institution which occupied the site of our present Royal Infirmary—and in the course of a few days upwards of 300 cases were under treatment. The circumstances were interesting in a sanitary point of view. The building had to be improvised as a hospital; and owing to the operations of reconstruction having been begun, the enclosing walls and railings were imperfect, and permitted of free access to the building on all sides. The inhabitants in the immediate neighbourhood were naturally alarmed. Another large school—Heriot's Hospital—was separated only by a thoroughfare from this smallpox hospital; and almost as near we had George Square, inhabited by highly respectable citizens; and, as I have mentioned, a busy thoroughfare formed the northern boundary, while to the south there was a favourite walking resort of the inhabitants. A more crucial test to determine the distance to which the germs of smallpox could be carried through the atmosphere can hardly be imagined; and yet in not a single case was the disease propagated from the hospital to the neighbouring houses. Not a whisper of a suspicion was ever heard that this hospital acted as a centre of infection to any portion of the citizens. Its neighbourhood enjoyed an unusual immunity from the disease; and as the ratepayers were on the alert, and Edinburgh has an active local press, in which the citizens find ample space to ventilate any grievance, I am sure you will agree with me that had any case been traceable to proximity to the hospital, the public and the authorities would have quickly heard of it. This remarkable immunity was dwelt on by the late Sir R. Christison at the last meeting of the Social Science Congress at Edinburgh. It was effected by a very simple arrangement. The hospital was regarded as an infected and highly dangerous place, and was, therefore, subjected to a rigorous but easily arranged system of quarantine. The assistance of the police was obtained, and a special set of officers—both in uniform and as detectives—was told off to watch the building by night and by day. Our daily experience showed us how easily, under ordinary precautions, smallpox hospitals become centres of infection to the neighbourhoods in which they are placed. The anxiety of convalescents to communicate with the outside world, the determination of relatives and friends to maintain correspondence with patients and nurses are such, that special means of prevention must be employed. This, as our experience showed, can only be satisfactorily accomplished with the assistance of the police. An argument, were one required, to show the importance of amicable relations subsisting

between the sanitary and police authorities; generally these are of a highly strained character, as I have often observed in the course of my official inspections in other districts, and leading to a direct conflict of officials who should act in concert instead of opposing the public interest. The late Dr Letheby complained to me of this antagonism in his own district in London. I am convinced that the only solution of the problem is to conjoin the offices of medical officer of health and surgeon of police in one person,—a junction which would not only furnish the health-officer with a large number of assistants, but by the consequent increase of his salary, would enable him to discharge the delicate duties of his office untrammelled with the cares of practice.

This I hold to be essential. All medical officers of health should be relieved from private practice. And nothing would bring about this desirable consummation sooner than the adoption throughout the length and breadth of the land of a system of notification of infectious diseases. No member of our profession can allow with any equanimity the house of his patient, and possibly his patient himself, to be visited by a rival practitioner, who, let him act with the greatest prudence, must at times give rise to suspicion, and may find to his annoyance that some simple phrase or act has been so misrepresented as to give grave offence to his fellow-practitioner. The remedy for such a danger is, as I have said, the introduction of a general system of notification in Scotland, which would at once elicit from the profession such a decided expression of opinion as would compel local authorities to relieve their health-officer from the cares of practice; and by the judicious combination of districts, would secure for these officials a suitable compensatory salary. An additional check against sanitary officiousness is the phrase which has been introduced by our municipal authorities into our intimation card, putting it in the power of every practitioner to solicit or to retard the action of the sanitary authorities. The phrase "No immediate attention is required," if allowed to remain, informs the authorities that no necessity exists for their interference at that juncture; while if the word "No" is deleted, then action is at once invited.

This is represented by the opponents of the system as an abnegation of the powers of the sanitary authorities, and as placing them and their officials too much at the beck and call of the medical profession. Instead of regarding this, gentlemen, as any objection, I look upon it as one of the strongest arguments in favour of our method. The sanitary authorities must not attempt to override the profession. I hold strongly the opinion that every medical man, by assuming such an official position as health-officer, surrenders, so to speak, his individual liberty, and becomes a servant of the general public, and, I need hardly add, of his professional brethren. I regard it as no indignity to be informed by the youngest member of the medical profession of the existence of

a nuisance, and to be called upon by him to remove it. I certainly do not consider it necessary to visit the house of a patient when it has been certified to me by a qualified medical man, that satisfactory steps have been taken to isolate the patient, and to prevent the spread of the disease. To act otherwise would be, in my opinion, to provoke conflict with my professional brethren, which would certainly end, in such an enlightened centre of medical science, in my dismissal, and, what would be worse, the discredit of sanitary science.

In country districts, where the existing medical practitioner has no local medical opinion to back him up, such intrusion as I have referred to is intolerable, and has, in my official experience, led to the serious detriment of the public health from the refusal of medical men to afford information to an official who had given them just grounds of offence.

Lastly, gentlemen, I have but one other topic with which I venture to trespass for a few minutes on your patience, and this has reference to statistics, and specially the statistics of "Uncertified Deaths." By these I mean cases of fatal illness in which no medical man has been summoned. I hold very strongly that every such uncertified death should be made the subject of special investigation. On inquiry it is found that Scottish towns generally show a large percentage of these deaths. In Edinburgh last year, on the other hand, no uncertified death was recorded in the various registers of our city. Acting on the principle I have just mentioned, whenever a party comes to register such a death at any of our local offices, he is at once referred to the police authorities, and I am called on to visit and report. Should the case be one, *e.g.*, of old age or long-standing illness, which has not necessitated the regular attendance of some medical man, where sudden death has occurred, I feel myself justified, after due inquiry, and in some cases after a partial post-mortem examination, in certifying the cause of death as apparently due to old age, or otherwise, as the case may be. On the other hand, should the case present elements of uncertainty, the assistance of the police is at once invoked, and a report of the circumstances is drawn up and submitted to the public prosecutor. On that official is now thrown the onus of determining what future action is to be taken. Citizens, especially among the lower orders, too often show a carelessness and dilatoriness in summoning medical aid in the case of sudden illness; and with the terrible temptation suggested by our burial clubs and societies, the fear must be expressed that a too easy explanation may be given of a large number of these uncertified deaths among young children. These are always carefully examined into by the police; in many cases, if no prosecution follows, the relatives are duly warned of the risk they have undergone, and generally they have been subjected to such a considerable amount of trouble and delay as to teach them the wholesome lesson not to

neglect in any future case to avail themselves of the many sources of medical assistance afforded in our city.

And now, gentlemen, in conclusion, I have the satisfaction of handing over to my successor in this Chair, to be appointed at this meeting, the Presidency of a Society which is in a highly flourishing condition. The secretaries assure me that during the two years I have held office no fewer than fifty-four members have joined the Society, a larger number than in any previous two years of its history. Much of this success has doubtless been owing to the high-class character of the papers read, and to the generally interesting nature of our meetings; but much also is due to the officials, including my Council and the Secretaries, Treasurer, and Editor, to all of whom I beg thus publicly to tender my acknowledgments. Our time-honoured Society has, so to speak, taken a new lease of life. It supplies a want in our great Medical School; and when I look at the roll of our members, comprising the *élite* of our profession, and especially those younger men who have already given high promise of increasing the reputation of Edinburgh as a scientific centre of medical education, I have no fear of its continued prosperity. Gentlemen, I thank you for the honour you did me in electing me to this Chair, and for the kindness with which you have listened to my parting words.

II. — CASE IN WHICH DIAGNOSIS WAS COMPLICATED BY A COINCIDENCE.

By JOSEPH BELL, F.R.C.S.E., Senior Surgeon to the Royal Infirmary.

J. M., an ostler, was admitted to Infirmary under my care on 10th Nov. He had received a severe blow on the forehead, and the side of his head had struck the curbstone. He was insensible; breathed stertorously; blood was oozing from his left ear. He had a severe bruise with much effusion over left parietal eminence, and a distinct deeply depressed fracture of temporal and parietal bones in a line running from the left ear to the bruise described above. I was sent for to trephine him. However, after a careful examination, I came to the conclusion that the sopor was chiefly due to alcohol, and that while the bruise and the ear injury were recent, that *the fracture was an old one*. This was received by all present with considerable incredulity, so I put the following case. Use the stomach-pump and shave the head. If the patient is not intelligent in two hours, and you find no old scars on head, send for me again, and I will cut down on the fracture.

The stomach was emptied, and he soon became quite sensible. The head was shaved, and two very large old scars were found, and the patient has made an uninterrupted recovery.

III.—SUBJECTIVE SYMPTOMS IN EYE DISEASES.¹

By GEORGE A. BERRY, Ophthalmic Surgeon, Royal Infirmary, Edinburgh.

II. PAIN.

WHENEVER pain is complained of in the eye, it is only right that an examination should be made of its functions, with reference not only to its visual capabilities, central and peripheral, as well as qualitative and quantitative, but also to the dioptric, accommodative and muscular arrangements on which the normal performance of these functions greatly depends.

To those unaccustomed to make such examinations, and who, although able to use the ophthalmoscope, have not sufficient practice to justify them in placing complete confidence in the result of their own examinations of the fundus of the eye, there is often a feeling that the pain complained of by their patients is due to some deep-seated inflammation or other pathological process which they are unable to detect. The fact is, however, that pain in the eye, unaccompanied by appearances of external inflammation, or more or less sudden visual defects, is only very rarely the result of changes which require the use of the ophthalmoscope for their detection; and, indeed, as a general rule, always excluding the cases where there is a visible inflammation, the greater the pain complained of, the less likely is it to depend upon any pathological condition at all which can be referred to the eye.

In making an analysis of the causes of pain, we may distinguish two great groups: 1, non-inflammatory; 2, inflammatory pain.

In a large proportion of the cases of pain unaccompanied by inflammation, the pain only comes on or is at all severe when the eyes are used for work near at hand, such as reading or sewing. In other cases, again, the pain is independent of the use of the eyes. The name which is generally given to the non-inflammatory pain associated with the use of the eyes is *asthenopia*, which literally means a want of power in the eye to perform its functions.

We may distinguish three forms of *asthenopia*: 1, accommodative; 2, muscular; and, 3, nervous or retinal *asthenopia*.

Accommodative Asthenopia.—*Asthenopia* is generally inferred to be accommodative when the condition of the eyes is such as to necessitate and bring into action a stronger stimulus to accommodation than is required normally under similar conditions. Cases of hypermetropia and of paresis of accommodation come under this category. The latter, besides being much less common than the former, are not so often associated with *asthenopia* as might be expected, greatly because attempts at accommodation are not persisted in to the same extent as in hypermetropia. In hypermetropia the *asthenopia* is probably more frequently due to

¹ Continued from February 1882.

the maintaining of an abnormal relation between accommodation and convergence of the optic axes on the object fixed than to the keeping up of a larger proportion than normal of the available accommodative power, though probably to some extent to both these circumstances combined. That such is the case is shown by the fact, that when a hypermetrope squints he is freed from asthenopia, although making use of as much accommodation as others with the same degree of refractive error, who do not squint. The two impulses, to convergence and to accommodation, are associated impulses; and it is owing to there being great individual differences in the extent to which they can be dissociated—differences which may be partly acquired and partly inherited—that a degree of accommodation can be maintained by one hypermetrope without asthenopia and without the sacrifice of binocular vision, which in another hypermetrope to the same extent would be impossible for any length of time. Certain cases of periodic squint show, indeed, how close some have to approach to the limit of their power of dissociating the two impulses. If they happen to have equal and good visual acuity in both eyes, the desire for binocular vision, or what is called the impulse to fusion of the retinal images of the two eyes, is sufficiently strong to effect the dissociation under favourable circumstances, that is, when the nervous centre is in a vigorous state. But the maintaining of the fusion gives rise to asthenopia, and brings about a tiring of the nerve centres, which renders the continuance of the effort impossible; and it is every now and then (sometimes at such regular intervals as to have been looked upon as of the nature of ague by some writers) relinquished for the alternative of binocular vision with convergent strabismus (mostly with diplopia), to be resumed again after the period of rest which this state affords. Every now and then one sees cases of strabismus occurring suddenly after debilitating illnesses, poisoning of the nerve centres, or lassitude from any cause, which can only be explained by assuming that the tendency to fusion, or what has been called the breadth of fusion, has been diminished; and in such cases the dissociation of the impulses to accommodation and convergence has been constantly maintained hitherto generally with more or less asthenopic pain. In them, although binocular vision for near objects is effected with difficulty, the difficulty is, either from great existing breadth of fusion, or from the less extensive or constant call made on the store of power at the disposal of the individual, not sufficiently great to necessitate his giving up the advantages of binocular vision until something unusual occurs, such as already signified, to weaken the nervous centres. A much more common cause of sudden squinting is ulceration of the cornea, the obvious explanation of which is the previous existence of predisposing causes, which immediately assert themselves when the value of binocular vision, from defective vision of the one eye, becomes lessened. When this takes place

in adults, there has usually been asthenopia previously, though not always, as the squinting position may be due merely to the assumption of the position of muscular equilibrium, otherwise easily overcome in the interest of binocular vision. The pain complained of in accommodative asthenopia is not generally very severe, and is more of a tiring nature. It is referred sometimes to the eyes, but more commonly to the supraciliary regions of the forehead, sometimes to the whole head. The obvious indication for treatment is to assist the accommodation by the use of convex glasses; and for this purpose, in cases of hypermetropia, it will generally be found practically sufficient to correct the manifest portion, enjoining, however, a constant use of the suitable convex lenses, both for distant and near vision. An attempt to correct the total, or nearly the total, amount of the hypermetropia is often unsuccessful, as, the spasmodic tendency to accommodation which exists in hypermetropic individuals, the more forcibly the younger the age of the individual, produces a combination with fully correcting glasses equivalent to myopic refraction, and the indistinctness of vision thus caused leads frequently to the laying aside of the glasses altogether—distinct vision, even with asthenopia, being preferred to short-sight and no pain. But in addition to the short-sight thus induced, it not unfrequently happens that the full correction, though theoretically calculated to relieve the symptoms of asthenopia, fails to do so, in which case the individual is altogether worse off with, than without, his glasses. The fact is, that accommodative asthenopia, though sometimes disappearing at once under the use of proper convex lenses, is often at first little influenced by the altered and improved conditions which they bring about; there is, as it were, a sort of neurosis set up, which only slowly disappears under the more favourable conditions. Sometimes, indeed, the cause of the pain from the first has not been any accommodative difficulty, though the existence of hypermetropia has justified such a suspicion, but has been more of the nature of nervous asthenopia.

Muscular Asthenopia.—Pain primarily due to over-exertion of the ocular muscles consequent on abnormal attachments, deficiency of power, or abnormal positions of muscular equilibrium of the two eyes, is certainly much less frequent than was at one time supposed. We may, however, include under the head of muscular asthenopia the pain or discomfort which arises from two conditions: 1. A defective impulse to convergence, or, as it is sometimes called, insufficiency of convergence; 2. A *high* degree of latent divergence or convergence of the visual axes, which necessitates the keeping up of an abnormal strain on the antagonistic muscles in the interest of binocular vision. Insufficiency of associated movements other than convergent are, as far as I have observed, not associated with asthenopia.¹ An insufficient impulse to con-

¹ I have also met with three curious forms of paralysis of muscles used in

vergence may be of the nature of a true associated paralysis—paresis of convergence. Such cases are certainly uncommon, but I have seen several, all giving rise, owing to the effort required to avoid diplopia, to asthenopia. But much the most common cause of convergent insufficiency, often wrongly called insufficiency of the interni, is the defective impulse which arises from want of support from the associated impulse to accommodation met with frequently in the higher degrees of myopia. In these cases sufficient convergence for the requirements of binocular vision is with difficulty maintained, owing to the distance from the eyes at which vision becomes distinct approaching much nearer the extreme limit in which the two axes can be converged than under normal conditions of refraction. The condition is exactly the converse of that described as causing a strain in hypermetropic individuals there is an abnormal dissociation of the two associated impulses in the interest of binocular vision; but whereas in hypermetropes the accommodation is strained without corresponding convergence, in myopes the convergence is exerted independently of accommodation. And here again we meet with great individual differences in the amount of asthenopic trouble to which the necessity for dissociating the impulses to convergence and accommodation gives rise—differences which are partly acquired and partly hereditary. A person who becomes rapidly highly myopic, or a myopic individual whose more immediate progenitors have not suffered from the same abnormal state of refraction, is more likely to experience asthenopic difficulties than one in whom the myopia has only been slowly progressive, and in whom it is inherited from perhaps a good many generations of ancestors. There is, however, a tendency to the abnormal relation between convergence and accommodation necessarily existing in myopes with binocular vision and uncorrected refraction becoming confirmed, just as in hypermetropes there is a portion of the hypermetropia rendered latent by involuntary ciliary contraction; and this circumstance explains why it is often difficult, or even impossible, for myopic individuals to wear at first the glasses which fully correct their refraction, more especially if the first correction is made pretty late in adult life. It is easy to satisfy one's self that the smaller power of convergence in many cases of myopia is not due to any weakness of the interni muscles, and therefore, as already remarked, wrongly expressed by the term insufficiency of the interni; for we find that the associated movements to the right and left are effected with the greatest ease, to an extent which calls for a far greater deviation inwards of either associated movements, unaccompanied by asthenopia. In one case, complete paralysis of both interni; in another, complete paralysis of both externi, which remained unaltered for four years, and ended in insanity; and, lastly, two cases of what has been called by Hutchinson ophthalmoplegia externa. The cases cited by Hutchinson are of two distinct origins: one depending upon gross cerebral changes, possibly of a syphilitic nature, and not so very rare; and the other to which I refer depending on localized degenerative cerebral lesions.

eye than is possible when attempts at convergence are made. The nature of the pain in this kind of muscular asthenopia is much the same as in accommodative asthenopia. It is referred sometimes to the eyes, but more frequently produces a diffused tired feeling in the forehead and other parts of the head.

Asthenopia, again, which is not accommodative, or muscular in the foregoing sense, may nevertheless be assumed as being connected with the muscles when we find any *very considerable* degree of latent deviation of the axes, or when as a congenital defect or as the result of operation, an abnormal position of attachment necessitates, under all circumstances, a very much greater muscular action in order to bring about a certain position of the eye. I say *very considerable*, because one often sees latent divergence where there is no asthenopia; and it is only when the point of departure, so to speak, of the action of the interni is very much displaced that this condition is likely to give rise to difficulties. In latent convergence the difficulties are greater, owing to the smaller tendency altogether to deviation of the axes of vision. A latent convergence must increase the difficulties of a hypermetrope, and is one of the factors which (along with monocular amblyopia, insufficient breadth of fusion, and in many cases hypermetropia) lead to convergent strabismus. Conversely, a hypermetrope, whose eyes in their position of equilibrium¹ would diverge, is less likely to be troubled with asthenopia. In some cases, then, a condition which might be supposed to give rise to muscular difficulties, and consequently asthenopia, acts rather in a manner tending to counteract asthenopia. The pain in what might therefore be called true muscular asthenopia, as being due to muscular fatigue, and not strain on the nervous mechanism called into play in associated muscular action, is most frequently referred to the eye itself, giving rise to a tired, aching feeling of the eyeball.

The indications for treatment in muscular asthenopia are for the form depending on insufficiency, strengthening of the nervous impulse to convergence, or occasionally to divergence, either by the selection of suitable concave or convex glasses, or, in the rarer cases, where the cause is more of a true parietic nature, by the use of means calculated to restore, if possible, the vigour of the nervous centres; it is seldom

¹ By the position of equilibrium is meant the position of the two visual axes when one eye is fixing an object at a distance (without exertion of accommodation), or staring vacantly into space, whilst the other is occluded. The position of equilibrium is not easily determined with certainty in hypermetropia, because of the accommodation brought into play even when fixing a distant object. Under atropine, too, there must be, when an attempt is made to see anything distinctly at a distance, an *impulse* to accommodation, with which a degree of convergent movement is naturally associated, so that the position of the occluded eye is not that of true equilibrium. Thus parallelism of the axes, when one eye is occluded and the other fixing a distant object, may, and often does, mean divergent equilibrium; and convergence under similar conditions does not necessarily imply a convergent position of equilibrium, which may be parallel or even divergent.

that any operation is called for or likely to do good. On the other hand, the more truly muscular cause of asthenopia may be obviated by suitable operative measures, the most successful of which is the advancement of the weakened or disadvantageously placed muscle. It is not always, however, that this treatment which seems to be indicated is effectual in removing the pain, notwithstanding the apparent¹ or real improvement to be found in the muscular relations. This is no doubt owing to the neurosis set up by the long continued previous conditions, and which cannot disappear at once; or it may be owing to the asthenopia being purely nervous, and unconnected with those muscular abnormalities which might be supposed to give rise to it.

Retinal Asthenopia.—Pain in the eyes, often described as right at the back of the eyes, unconnected with any accommodative or muscular abnormality, and coming on, often very severely, at longer or shorter intervals after use of the eyes for reading, etc., is extremely common. Often the condition is associated with more or less sensitiveness to light. In many, and indeed most, of these cases there is absolutely nothing to be found in connexion with the eyes, at any rate, to account for the persistence or severity of the symptoms. Either the strain on the attention becomes soon too overpowering, or the retina itself is hypersensitive and easily tired; at all events there is some weakness in the tone or capabilities of the nervous mechanism of vision. Such a state of nervous asthenopia is probably frequently of reflex origin, though the region from which the afferent stimulus proceeds is not often known. It is more common in women than in men, which has led some authors to ascribe it to uterine irritation, and even to describe a particular form of chronic inflammation with which it is associated. The causal connexion between the two, as a matter of constant or even frequent occurrence is, however, in the highest degree improbable. Intestinal irritation also appears sometimes to give rise to this form of asthenopia. In a great number of cases—and I am disposed to think in by far the greatest number—the pain is due primarily to over-exertion of the eyes at a time when, after a debilitating illness, or a state of malnutrition, anæmia, etc., the nervous tone is below par. The pain thus originating becomes chronic, a sort of neurosis or habit, and may become so distressing as to render reading impossible for more than a few minutes at a time. Sometimes the condition of the eyes themselves is such as to induce asthenopia by over-use, such as congenital

¹ I have said apparent as well as real, because it often happens that the rest afforded by the operation is sufficient to permit of a greater momentary exhibition of muscular power than could be elicited before; and, besides, there is a considerable personal error in the estimation of the power of a muscle, more especially if use be made of any instrument, such as a dynamometer, from which one expects accurate results, just as there is a possible personal error in the estimation of refraction by means of the ophthalmoscope, or in the determination of the exact limits of peripheral vision.

amblyopia and astigmatism, where the difficulties in deciphering small type are considerable, and therefore accompanied by abnormal strain. Indeed, one can bring on the same kind of feeling in one's own eyes either by attempting to read small type beyond the limit of distinct vision, or with an illumination barely sufficiently powerful for the purpose, and that independently of over-exertion of convergence or accommodation. Individuals suffering from this form of asthenopia generally take, sooner or later, to the use of blue or dark spectacles; and by so doing merely increase the sensitiveness of their retinae. Such spectacles should only be employed in cases of deep-seated inflammation, in which the tempering of the light passing into the eyes is one important means of complying with the indication for rest, or under conditions of excessive glare from powerful sources of illumination or reflexion. Many cases of retinal or nervous asthenopia are amongst the most severe and rebellious that one is called upon to treat. Cold-water douches, and iron internally, are indicated in most cases; and when there is absolutely nothing abnormal to be discovered in the eyes, an energetic attempt should be made to break the habit of giving up reading as soon as the pain comes on. This can only be done gradually, but is successful in many cases.

Pain independent of the use of the eyes is often complained of, and generally of a neuralgic character, and more or less intense. The diagnosis is easily made when there are points of special tenderness on pressure round the eye. Often the eyeball itself is tender to pressure. Occasionally, no doubt, the pain is reflex, but the origin of most cases is a hypersensitiveness of the supra- and infra-orbital branches of the fifth nerve, caused generally originally by exposure to cold. Keeping the head warm, especially at night, and the use of iron, arsenic, and quinine, is the line of treatment indicated. In rare instances an intraocular tumour may give rise to pain before having produced inflammatory or visual changes, to be detected on superficial examination, though hardly before causing some limitation of the field of vision.

Inflammatory Pain.—Of pain produced by inflammation, or by some definite lesion other than inflammation, there are two pretty definite types: one in which the pain becomes much more severe under the influence of light, in which there is then more or less marked photophobia, and the other in which photophobia is absent. The co-existence of photophobia with the irritation of sensory nerve filaments in the eye is an instance of reflex, or it may be direct transference of stimulation from a nerve of common to a nerve of special sensibility, which, so far as I am aware, has no absolute parallel anywhere else in the body. There is no reflex in the ordinary acceptation of the term; that is to say, the irritation of fibres of the fifth nerve does not lead to stimulation of those of the optic nerve so as to give rise to subjective light sensations, but the transference is of such a nature as to cause the functional activity

of the optic nerve, which under ordinary circumstances is painless, to be associated with pain, sometimes very intense. We do not find that irritation of the sensory fibres supplying the more external parts of the ear or nose give rise to painful hearing or smelling. Certainly a very loud or shrill sound, or even a moderately loud sound, after a prolonged period of silence, produces a painful or disagreeable sensation which causes one more or less involuntarily to stop up one's ears, just as excessively strong light, or a light which, when the retina has become accustomed to it, is easily tolerated, when falling on the eyes after a period of darkness, is painful to the eyes, which therefore close against it. So far there is a parallel, but there is not the same hypersensitiveness induced, and accompanying functional activity of the auditory nerve, so soon as there is an unwonted stimulation of a few superficial twigs of the nerve supplying the ear with common sensibility, as is the case with the optic nerve immediately after some disturbance of or loss of superficial corneal epithelium. It is difficult to account for this sensitiveness only caused by light, as the optic nerve itself is apparently not very sensitive; there is, at all events, no pain caused by inflammation of the optic nerve, and stimulation of the cut end after enucleation of the eyeball is generally hardly felt. On the other hand, we have seen in discussing nervous asthenopia that, under various circumstances not absolutely pathological, the mere use of the eyes is accompanied by pain, so that it is not easy to avoid the conclusion that somewhere or other, possibly in the retina itself, there is an intimate connexion between the optic and fifth nerves, whether acting through the vaso-motor system or in what manner can at present only be conjectured. Occasionally the increased irritation of sensory nerves caused by the action of light is spread over a wide area, one curious example of the wide diffusion being the sneezing which is frequently met with in children on forcibly opening their eyes, firmly and persistently closed from photophobia. The irritation also causes an immediate hypersecretion of tears, photophobia being always accompanied by lacrimation. The conditions giving rise to photophobia are generally some lesion or inflammation of the cornea, iris, or ciliary body, though the degree of photophobia varies much even in apparently similar conditions. Traumatic, phlyctenular, and interstitial keratitis and iridocyclitis are the most frequent conditions in which we meet with more or less photophobia. Deep and extensive ulcers of the cornea are not so much complicated by this symptom, though often giving rise to great pain, probably because they destroy without keeping up a prolonged irritation of the superficial nerve endings in the cornea. Some of the most intense cases of pain as well as photophobia accompany slight abrasions of the cornea, caused, for instance, by a scrape from a child's nail, or a twig of a bush or tree, or in some other way; and there is a tendency for the irritation thus set up to give rise to recurrent attacks

at intervals, in which the pain is often quite as severe as at the time of the injury. Photophobia once set up is often maintained, more owing to the sensitiveness of the retina which follows prolonged exclusion from light (as in retinal asthenopia by the constant use of dark spectacles or residence in darkened rooms) than to the exciting cause of the local stimulus. This is especially the case with strumous children, who have been suffering from superficial (phlyctenular) keratitis, where the photophobia is often seen lasting long after there is any sufficient cause to account for it on any other supposition. Here it is that efforts to break the habit, such as the shock to the nervous system by immersing the head in cold water, the forcible opening of the eyes, or the use of atropine or cocaine, are often almost immediately efficacious. So that although photophobia may be primarily a provision of nature for the protection of the eyes, it often becomes a distinct pathological condition, owing often to misguided treatment, actuated as a rule by over-anxiety on the part of parents or others. The action of atropine, in often diminishing pain and photophobia, is mostly due to its direct anodyne effect,—to some extent, however, and more especially in interstitial keratitis and other deep inflammation, to paralyzing the movements of the iris.

The pain of iritis is often very intense, and affords a good gauge of the severity of an attack. It is generally most intense at night, and radiates over the region supplied by the supra- and infra-orbital branches of the fifth nerve, being often complained of as at the side of the nose. Although the accompanying photophobia is rarely as intense as in some more superficial inflammations, the withholding of light renders the pain less severe by keeping the iris at rest, and this, along with the use of a mydriatic for the same purpose, is the most important indication for local treatment, both as regards the alleviation of pain and the avoidance of complications which may afterwards be sources of greater or less danger for the eye. In purely rheumatic forms the pain is often checked at once by the use of salicylate of soda internally, given frequently (7–8 grains every hour). In simple iritis there is no particular sensitiveness of the eye to touch. When this exists in an attack of iritis, it indicates a participation of the ciliary body in the inflammation (cyclitis); and the degree of sensitiveness, sometimes intense, affords the only sure indication of the severity of the accompanying cyclitis.

In simple choroiditis, except in cases where the intraocular tension becomes distinctly increased, there is but little pain or photophobia; sometimes a dull aching pain at the back of the eye is complained of. Suppurative choroiditis, leading to panophthalmitis, gives rise, however, to dreadful suffering, the intensity of the pain being, as far as inflammatory conditions of the eye are concerned, only equalled, if at all, by that of acute glaucoma or of acute inflammation of the orbital tissues; but this is mostly

due to the participation of other tissues in the inflammation, and particularly to the tension in the eye caused by the exudation.

The pain of extensive or deep ulcers or abscesses of the cornea, and especially that form of septic inflammation which is accompanied by a deposition of pus in the anterior chamber (hypopyon), is often very severe, as is the case always where supuration takes place in tense tissues. The relief of tension caused by opening such a septic abscess, though eventually causing great relief of pain, produces at the time often the most agonizing suffering.

The pain of glaucoma is very variable, just as are the symptoms of that disease altogether. It is probably greatly due to irritation of the sensory nerves from the accompanying increase of intra-ocular tension, though increased tension alone is not always associated with pain. In acute inflammatory glaucoma the irritation of the fifth nerve is so great as to extend to all its branches, and gives rise to such severe general symptoms as to be apt to divert attention from their original source and lead to errors of diagnosis. Such a mistake is all the more likely to occur when there has previously been blindness of the eye from which the pain proceeds.

One of the commonest, and at the same time most obscure, sources of pain in the eye—seldom, however, except when acute, very severe—is conjunctivitis. In the acute forms it is a sharp burning pain, very different from the diffuse neuralgic pain of iritis, and giving rise at the same time to the sensation of there being dust or sand in the eye. When chronic, it is more a dull aching pain which is complained of. This is often due to the slight glueing together of the palpebral and ocular conjunctiva by the glutinous secretion which is so common in many cases, and in these cases the lids feel heavy and tired. Frequently, though, there is no heaviness of the lids arising in this way, and it is often difficult to imagine how the slight amount of visible inflammation should be accompanied by so much discomfort. In this respect, moreover, there is a vast difference in different individuals, a difference greatly due to the sensitiveness and occupation of the individual. A degree of conjunctivitis, which in a labourer would be quite unheeded, will often cause great distress to a lady who, besides being naturally more sensitive, has more leisure to brood over her misfortunes.

From the preceding short sketch of one of the most common and important subjective symptoms met with in affections of the eye, it will be evident that the existence of pain will often lead one to the detection of pathological conditions which might otherwise escape notice, owing to the absence of other striking symptoms; and that, besides, the nature and severity of this symptom may be of use in leading one to the diagnosis of the site of, as well as the course taken by, inflammatory diseases of the eye, even though it may in many cases be of less importance than the accompanying objective signs.

IV.—RARE CASES OF MALIGNANT DISEASE OF THE FEMALE SEXUAL ORGANS.

Observed in the Buchanan Ward, under Professor Simpson, by
FOURNESS SIMMONS, M.B., Buchanan Scholar.

(Read before the Edinburgh Obstetrical Society, 8th July 1885.)

(Continued from page 558.)

CASE III.—*Sloughing Sarcoma of Posterior Vaginal Wall; Removal; Death a Month subsequently from Recurrent Hæmorrhage.*

A. M., æt. 19, married, was admitted into the Buchanan Ward on 27th November 1884, on the recommendation of Dr Freeland Barbour, complaining of a sanious discharge from the vagina, and pain in the lower part of the abdomen.

Her illness commenced seven weeks previously to admission by what appeared to be an ordinary menstrual flow, but the discharge continuing copiously for a fortnight she grew alarmed, and sent for a medical man, who gave her some medicine which greatly checked it. This continued uninterruptedly until five days before admission, when a sudden gush of blood took place from the vagina, and this recurred the following day whilst straining at stool, when, for the first time, it was noticed to have an offensive odour. A fortnight since patient noticed a small lump in the vagina, which came down during defecation, but never caused any pain. About a month after the onset of the discharge she was troubled with pain in the left iliac region, extending to the small of the back, and down the left leg as far as the knee.

The patient was anæmic to the last degree; expression careworn and anxious. There was a loud *bruit de diable* in the neck, and loud murmurs (probably organic) in the mitral and auricular areas. On everting the posterior vaginal wall with the finger in the rectum, a circumscribed swelling about the size of a walnut is brought into view, covered with vaginal mucous membrane, except on the lower and left side, where there is an irregular patch about the size of a sixpenny piece, which presents a blackened, sloughy appearance, and has an irregular surface. The margins of this patch are irregular, somewhat sharply defined, especially on the right side, but neither thickened nor hard. To the left of the posterior raphe of the vagina the mucous membrane has a bluish, patchy appearance, alternating with its hyperæmic condition around. This tumour had a solid consistence, not nodular; surface smooth, except over the sloughing patch alluded to, and was readily pushed outwards from the vagina by coughing or forced expiration. The vagina elsewhere, cervix, and uterus are healthy.

On admission (27th Nov.) she was ordered a hypodermic injec-

tion of ergotin (3 grs.) each night, and the following mixture internally :—

R Tinct. ferri. perchlor., . . . ʒiij.
 Tinct. digitalis,
 Acid. phosphoric. dil., . . . ʒā ʒj.
 Aquam ad., . . . ʒvj.
 Sig. ʒss. ter in die post cibum.

30th November.—Copious viscid, yellowish, very foetid discharge from vagina. No hæmorrhage.

2nd December.—Profuse discharge of blood from vagina during defecation. Checked by pressure of fingers in the vagina and rectum. Professor Simpson constricted the base of the tumour by two pairs of dressing forceps, the handles of which were kept in apposition by silver wire wrapped round them. Silver wire sutures were then passed below the tumour, which was then removed by the knife. The edges of the wound in the vagina were brought into accurate apposition by the silver wire, and dressed with powdered iodoform.

11th December.—Stitches removed. Incision healed, and looking quite healthy.

19th December.—Wound firmly cicatrized. Vaginal discharge quite ceased. Patient appears perfectly well, with the exception of the general weakness due to the anæmic condition, produced by the loss of blood previous to the operation.

From 23rd to 31st December unfortunately hæmorrhage repeatedly recurred, at times very profusely. During this period the treatment consisted in the administration of hæmostatics and local plugging of the vagina, and may be summarized as an unsuccessful attempt to restrain the hæmorrhage, to which the patient eventually succumbed on the morning of 1st January 1885. No autopsy was allowed.

Viewed under the low power, the section of the tumour will be more conveniently described in two parts—superficial and deep. The superficial is almost entirely composed of cells of various size and shape, with numerous large and distended capillaries traversing its substance, with a few small hæmorrhagic extravasations here and there. In the deeper part, a few strands of non-striped muscular fibres and bloodvessels separated by large and extensive hæmorrhages are seen. In the Bismark brown stained specimen the characteristic cells of the tumour are brought out very clearly, at the expense, however, of its vascular arrangement. Under the high power (see Fig. 3) the tumour is seen to consist of large spindle-shaped and irregular cells, with large finely granular nuclei, infiltrating and separating bands of connective tissue. There are numerous capillaries running between the cells, the walls of which in some places are composed of a single layer of flattened connective tissue cells, whilst at other parts it seems to be absent. Here and there minute extravasations are seen. Many of the characteristic

cells have undergone peculiar degenerative changes,—swelling, becoming more or less rounded and granular, the nucleus undergoing the same changes.

Tumours of the vagina are acknowledged by all writers upon gynecology to be extremely rare. Sarcoma as a lesion in this position has only been quite recently described, and the infrequency with which it occurs may be inferred when it is not even mentioned in such standard works upon diseases of women as those of Gaillard Thomas, Grailly Hewitt, and West and Duncan; while Emmet, in his recent work, says sarcoma “is seldom, if ever, found below the internal os.”¹

The points of interest in this case appear to me to be—

1. The age of the patient, viz., 19 years. This is an unusually early age for malignant disease to develop, though it is true cases of sarcoma vaginæ have been observed in children three or four years of age.²

2. The small size of the tumour—being about the size of a walnut—and its sloughing condition.

3. The rapidity of growth of the disease, as shown by the fact that the patient was dead fifteen weeks after the onset of the first symptoms, despite the amelioration following the operation; and the microscopic examination of the tumour was still further confirmatory of this point. Cases of sarcoma of the female generative organs seem prone to run a prolonged course.

CASE IV.—*Malignant Disease of the Recto-vaginal Septum. Aspiration of Tumour per Vaginam; temporary Relief; steady Advance of Disease; Patient still Living.*

M. F., æt. 42, widow, was admitted into Ward XXIV. on 28th April 1884, on the recommendation of Dr MacLagan of Berwick, complaining of a constant bloody discharge from the vagina, pain and difficulty of micturition, and obstinate constipation. Her illness commenced in June of 1883, when the menses lasted fourteen days, and at the conclusion of the period she became very costive, feeling as if “something prevented the passage” of the fæces, and at the same time suffered slight pain during micturition. These symptoms improved slightly during the month following, but her menstruation became very irregular—at first there being an interval of fourteen days between the periods, later on only seven days, and during the three months previous to admission there had been a hæmorrhagic discharge daily, at all times necessitating the wearing of a diaper. During the past six months there has been obstinate constipation and pain and difficulty of micturition, there being a sensation as if something prevented the excreta being voided.

On examination, the perineum is entire but much distended,

¹ *Principles and Practice of Gynecology*, p. 507.

² Sänger, *Arch. f. Gyn.*, B. xvi.; and *Centralbl. f. Medic.*, 1880.

and forming the apex of a large body which projects forwards into the vagina and backwards into the rectum. The examining finger, per vaginam, could not reach the cervix owing to the bulging forwards of the posterior vaginal wall, which is distended over a firm, smooth, non-sensitive mass, clearly limited laterally, and, as far as could be made out, superiorly. Through the rectum a similar body is felt bulging its anterior wall backwards. Recto-vaginal touch shows this mass to be lying in the recto-vaginal septum, and about the size of a small cocoa-nut.

On 30th April 1884, Professor Simpson aspirated the tumour per vaginam, and drew off 8 oz. of almost pure blood, which quickly clotted. Microscopically it consisted of blood corpuscles. Subsequently the dysuria passed away, and the constipation was somewhat relieved; the hæmorrhagic discharge, though continuing, was by no means as copious as before. Dismissed, 21st May, expressing herself greatly relieved.

On 29th September 1884 she was readmitted complaining of floodings, with shiverings and pain in the left side. She stated that two months after her departure floodings occurred every three weeks, the discharge continuing copiously for a week. Constipation always aggravated the discharge, and she therefore habitually took aperients. The pain in the left side, which has lasted two months, has been worse of late. On physical examination, the condition of matters was almost precisely the same as on her first visit, with this exception—the perineum was not bulged, but there was a well-marked cluster of external piles.

On 10th October she was anæsthetized and examined; but after careful consideration, Professor Simpson determined that nothing of an operative nature could be done for her. The microscopic examination of the blood from the tumour showed spindle-shaped nucleated sarcomatous cells.

The subsequent treatment consisted in the regular administration of cascara sagrada for the obstinate constipation, gall and opium ointment locally for the piles, and occasional administration of anodynes to relieve pain. Micturition, at all times difficult, was frequently impossible, so that catheterization alone afforded relief.

Dismissed 13th November 1884. The mass in the septum somewhat enlarged. Since then I have not heard anything of her physical condition.

CASE V. — Second Case of Sarcoma of the Recto-vaginal Septum. Laparotomy and Aspiration of Tumour; Growth of Tumour upwards into Abdomen; Aspiration per Vaginam; steady Advance of Disease; Patient still under observation.

C. F., æt. 52, a widow, was admitted into Ward XXIV. on 12th October 1884, on the recommendation of Dr Freeland Barbour, complaining of pain and difficulty in the evacuation of the bowels

and in making water, with occasional retention of urine, and a copious watery vaginal discharge for the last three weeks. Three weeks ago she suffered from retention of urine, which has recurred six times during that period, and at the same time noticed a dull gnawing pain in the back, especially on exertion. She menstruated last a year ago.

On examination, per abdomen, a firm, smooth, non-sensitive, movable body is felt in the right inguinal region, apparently the fundus uteri. On vaginal and rectal examinations, the condition is almost precisely similar to that felt in the last case (Case IV.), but the combined recto-vaginal touch reveals the lower $\frac{2}{3}$ of an inch of the septum unintruded upon by the tumour, which is firm, smooth, non-sensitive, and, roughly, about the size of a foetal head, pressing anteriorly on the symphysis, and posteriorly on the sacrum.

As the condition was very similar to that in Case IV., it was determined to open the abdomen, to see if that would throw any light upon the diagnosis and treatment of the lesion. Accordingly, on 20th October, chloroform having been administered, Professor Simpson made an incision in the middle line, through thick, vascular abdominal walls, till the peritoneum was exposed, which was opened with scissors. The fundus uteri was seen raised upwards by a vascular-looking tumour, jammed in the pelvis, lying extra-peritoneally, and pushing upwards the pouch of Douglas (Fig. 4), which, on being tapped, yielded 12 oz. of dark blood, which, on

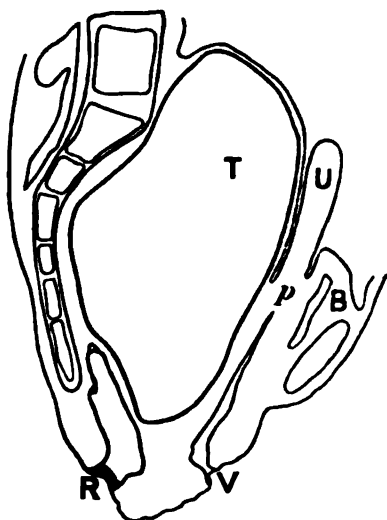


FIG. 4.—Diagram showing relations of tumour (probably sarcomatous) to the pelvic organs. T, tumour; U, uterus; R, rectum; V, vagina; B, bladder; p is opposite peritoneum of the pouch of Douglas. The tumour is sub-peritoneal, and pushes the peritoneum of the pouch of Douglas upwards and forwards.

microscopic examination, was seen to consist of red blood corpuscles

and a few *nucleated* attenuated cells. The peritoneal cavity was thoroughly cleansed with carbolyzed sponges, the edges of the incision accurately adapted, a dry iodoform dressing applied, and secured by sticking-plaster.

Per vaginam (after operation).—The tumour is to touch considerably smaller.

The temperature never rose above 99°·2 F.; the stitches were removed on the seventh day, and the wound found to have firmly cicatrized. The patient was up and about on the eighteenth day.

11th November to 9th December.—Urine has to be drawn off almost continuously. At times can make water with great difficulty and much pain.

11th December.—A firm, smooth, tender mass felt, occupying the hypogastric, and extending into the right inguinal region, superiorly and to the right of which the uterine fundus is felt. Tumour aspirated *per vaginam*. Apparently quite solid and very vascular, as pretty profuse hæmorrhage followed the withdrawal of the aspiratory needle, which was checked by packing the vagina with iodoform gauze.

18th December.—Ordered extr. cascara sagrada fluid (30 ℥ bis in die) for obstinate constipation.

15th January.—Since admission a watery, non-offensive discharge continues daily, though not profusely. Catheterization frequently necessary.

Per abdomen.—A somewhat irregular, hard, tender mass felt, extending upwards to about 2 inches from the umbilicus, surmounted by the uterine fundus as before. No bruit heard.

Per vaginam.—The tumour has a distinctly nodulated surface, extending from about an inch from the vaginal aperture upwards as far as the finger can reach.

19th January.—Dismissed with injunctions to continue the cascara sagrada (30 ℥ ter. in die).

10th April.—Has had much pain in the back and pelvis generally during the past two months; worse at night; relieved only by anodynes. At first paroxysmal, lately more continuous. During past month has had loss of power of right leg, and difficulty of walking.

28th May.—Much watery vaginal discharge during the past month,—still not offensive. Taking aperients and anodynes—the latter in increasing doses—habitually. There has been no retention of urine since March last. Has become much emaciated. Uterine fundus fell just below level of, and 2 inches to the right of, the umbilicus, raised upwards by the tumour, which has reached a level about 1 inch from the umbilicus. On its vaginal aspect the nodulated character of the swelling is still maintained.

These two cases (*i.e.*, Cases IV. and V.) are almost unique; for on a review of gynecological literature, I am able to find only one reference to this lesion in this position, *viz.*, Primary Sarcoma of

the Recto-vaginal Septum. This single case, briefly related by Dr P. F. Mundé,¹ is of such interest in this connexion, being so closely allied to the cases I have narrated, that I may be pardoned if I quote it in its entirety:—"A woman, 55 years of age, the mother of a number of children, had suffered from an enlargement of the abdomen, accompanied by pelvic pains, which during the past two months had become exceedingly severe. The tumour was very tender to the touch, and was found to lie in the recto-vaginal space, pressing the vagina forward and downward, and the rectum backward. It was tapped on two different occasions through the vagina, and a thick, bloody fluid, readily coagulable, was withdrawn, with a few flocculi of pus. The diagnosis lay between sarcoma with fluid contents and pelvic hematocoele. After three weeks, the tumour slowly increasing in size in the meantime, signs of septic infection appeared, and an opening was made through the vagina, and fresh coagula were discharged. The walls of the cavity were found to consist of irregular soft masses, which could readily be scraped out with the vaginal depressor. Microscopic examination showed it to be a round-celled sarcoma, with but little fibrous tissue. The temperature rose to 103°, and the patient died of exhaustion on the fifth day after the operation. Post-mortem examination showed that the disease was limited to the cellular tissue in the recto-uterine space, neither the uterus, the ovaries, nor any of the other organs in the pelvic cavity being in the least involved. He had not been able to find a similar case recorded in medical literature, although sarcoma of the uterus and of the ovaries was not uncommon."

I say that the two cases I have described were sarcoma, because I think the limited microscopic examination of the tumours we were able to make, point in both cases to sarcoma as the lesion; for in Case IV. spindle-shaped sarcomatous cells were distinctly seen in the blood, and in Case V. the *nucleated* attenuated character of some of the cells was very sarcomatous looking, and I only regret that at the time I did not have drawings made of the appearances. Further, that these tumours are undoubtedly malignant is, I think, abundantly supported by the clinical facts of the cases, and on a review of their history one is struck at once with the remarkable similarity of the clinical features, which may be summarized as follows:—

1. Both patients were of similar age—at the time of life when malignant disease is most prone to develop—viz., 42 and 50.

2. Bladder and rectal symptoms were the first to attract attention, viz., dysuria and retention of urine, together with constipation, both becoming aggravated on the advance of the lesion, and simply the results of pressure of the tumour.

3. Vaginal discharge—hæmorrhagic in Case IV., and watery in Case V.—non-offensive up to the present date, but always copious.

¹ *American Journal of Obstetrics*, vol. xvi. p. 946.

4. Pain, at first absent; later, becoming paroxysmal, and subsequently continuous, and relieved only by the regular administration of anodynes; in fact, just the character of the pain of malignant disease.

5. Still further, the steady advance of the disease, the aggravation of the symptoms, loss of flesh and cachectic appearance, which developed in these patients, are all in favour of a malignant neoplasm.

6. A remarkable similarity in the physical signs of the pelvis, the tumour being of the same consistence and in the same position primarily in both cases, though it is true in Case V. it subsequently became abdominal; and furthermore, its vaginal aspect came to have the nodulated feel of a malignant new growth. At first the contents of the tumours are fluid to some extent, for they can be withdrawn by aspiration; but subsequently they become quite solid, and the only effect of aspiration is to create smart hæmorrhage. The fluidity of the tumour at an early date was most probably owing to the fact that a portion of it was cystic—in fact, a cysto-sarcoma—and that hæmorrhage had occurred from the lining of the cyst wall. One might readily understand the occurrence of hæmorrhage in these cases, for during the act of defecation (especially in the existence of obstinate constipation, as in both these patients), the pressure of the bowel contents from behind, and the increased intra-abdominal force from above, would be quite sufficient to account for the rupture of badly supported vessels at their weakest points, which, even if only capillary oozing resulted, would in time suffice to fill a cavity such as I venture to think existed. Or, yet again, the fluid contents may have simply been the result of considerable blood extravasation into the midst of a solid tumour, which had a more loosely constructed centre. Or, it is possible, though quite improbable, that there was a cyst formation primarily, and a secondary sarcomatous growth around it, which, after invading the cyst wall, would allow capillary oozing to take place into the previous cystic contents for the mechanical reasons just mentioned. The subsequent solidity was simply the result of the removal of the fluid contents, collapse of the walls around, and possibly some increased cell proliferation from the irritant action of the aspiratory needle. These are, of course, mere theories—to my mind, plausible—though quite subservient to facts. The fluidity of the tumour does not prevent its being sarcomatous, for in the only case on record, we know there was an abundance of fluid contents.

The differential diagnosis requires to be made from two lesions—hæmatoma and carcinoma. Cases of hæmatoma in the recto-vaginal septum are very rare, though (1), the displacement of organs produced in Case V., the uterus being raised upwards into the right inguinal region; (2), the hard feeling of the tumour; and (3), the pressure symptoms resulting—dysuria, retention of urine, and constipation—were all in favour of this view. But in

Case V. the patient had passed the menopause, and the prolonged subsequent history of both cases makes this view quite untenable, as absorption of the swelling or suppuration in it would have occurred.

These cases were not carcinomatous in their nature for two reasons—(1), there was never any evidence of ulceration in these tumours; and (2), there was a total absence of secondary deposits. Both of these changes would have occurred if the lesion had been cancerous.

The treatment most advisable would appear undoubtedly to be aspiration of the tumour per vaginam (using every antiseptic precaution), as this accomplishes all that can be done by any operative procedure; for although there is very little danger following the abdominal section, its adoption cannot furnish us with any further reduction of the tumour. The plan followed by Dr Mundé in his case of making an opening into the tumour through the vagina and scraping out its interior did not have a favourable termination, probably because a septic condition arose before the operation, which was quite sufficient to justify its adoption. The relief thus given by any operative procedure is not very encouraging; and later on, when the symptoms become aggravated, they must be treated as in malignant disease generally, by anodynes. The obstinate constipation was best met by the constant use of *cascara sagrada* in gradually increasing doses.

V. — ON THE TREATMENT OF LABOUR DELAYED BY OBSTRUCTION AT THE PELVIC BRIM.

By SAMUEL SLOAN, M.D., F.F.P.S.G., Obstetric Physician to, and Lecturer on Clinical Obstetrics at the Maternity Hospital, Glasgow; Vice-President of the Glasgow Obstetrical and Gynaecological Society.

(Read before the Society, 11th November 1885.)

As a preliminary to my remarks on the treatment of labour delayed by obstruction at the pelvic brim, it will be necessary to define the nature of this obstruction. It must be remembered that an abnormally large head may be obstructed at a normal brim; but the treatment of such a case will not be materially different from that for a case of labour where an average head is detained at a contracted brim. Not to complicate the question unnecessarily, I shall also omit all mention of cases where the brim is contracted, only or mainly, in the transverse diameter. Of such cases I have met with several examples. They are not numerous, however, and I shall not consider them in this paper. Obstruction in the *conjugate* diameter, on the other hand, is a most common cause of delay in labour; and, to the treatment of delayed labour from this cause, so much attention has been directed by the ablest obstetricians of the day, that I feel almost

as if I might justly be blamed for "rushing in" where I ought to "fear to tread." But as I have, in the course of my hospital and other practice, seen many cases of this nature, and as the subject seems to me to require further elucidation, I feel that by bringing this question before you early in our first session, I shall be giving our young Society an early opportunity of showing that we can contribute something to the advance of Obstetric Science. Almost every Fellow of this Society must have some experience of cases of delay of the head at the pelvic brim, and I lay my views before you, not dogmatically for your acceptance, but tentatively for your criticism, though naturally with the hope that my propositions may meet with your approval.

I have mentioned that I wish to confine myself to the discussion of cases where the obstruction is in the conjugate diameter. Now in pelves with this deformity we may have the transverse diameter *proportionally* diminished, thus giving what is called a *generally contracted pelvis*. We may also have what is called the *flat pelvis*, this being either a normal pelvis flattened, or a generally contracted pelvis flattened. Arranging these, say, in the order of their interest, we have—first, the *simple flat pelvis*; second, the *generally contracted pelvis*; and third, the *generally contracted flat pelvis*. I have specimens of these three deformities here, and also a normal pelvis to compare them with. Clinically, these agree in the fact that the sacrum, projecting forwards at the brim, is generally more or less within reach of the examining finger; whereas in the normal pelvis the finger cannot possibly reach this projection. The consequent diminution of the conjugate diameter varies from a slight and clinically unimportant decrease to a contraction which may be called absolute. In the very slight degrees of contraction there will be no delay which cannot be overcome by the ordinary powers; whereas in the absolute contraction there will be such an obstruction as to prevent absolutely, under any circumstances, the passage *per vias naturales* of a viable child. Having thus narrowed our discussion to these limits, we necessarily eliminate on the one side, "patience," and on the other, Cæsarean section and allied operations, from the present inquiry.

I have just stated that this kind of contraction can be diagnosed by finding that the promontory of the sacrum is within reach of the examining finger, where the deformity is so decided as to come within the above limits. But how are we to ascertain to which of the three kinds of contraction a given case belongs? This is easily done, though not with the most desirable accuracy, by taking certain external measurements of the false pelvis, the only instrument required being an ordinary pair of callipers. We find that, in the normal pelvis, the distance between the anterior superior spines of the ilia is 10 inches; that between the crests at their broadest part being 11 inches. In a generally contracted pelvis these figures will be both diminished, and nearly equally

so, giving us, say, 9 and 10 inches respectively. When, however, a pelvis becomes flattened from before backwards, narrowing the conjugate diameter only, as in the rickety pelvis, the *relative* distances of these points become altered, the crests being approximated whilst the distance between the spines is increased. This will obviously hold in flattening of both the normal and the generally contracted pelvis. I find, it may be worth mentioning, that in the living subject these measurements generally appear under rather than over the true ones. A correct estimate of the internal conjugate is more desirable than attainable, for internal pelvimeters are not of much service. But a fairly accurate estimate of this diameter may be formed by measuring with the index finger of either hand the distance between the sacral promontory and the lower end of the symphysis pubis, and deducting from this *lower*, or *diagonal* conjugate, as it is called, about three quarters of an inch. The exact nature of pelvis we are dealing with is important when we consider the mechanism of labour in such cases; for the head, as we shall see, disposes itself, as a rule, differently at the brim in the flat from what it does in the generally contracted pelvis. We know that in normal labours the head at the brim is found, as soon as labour pains have begun to tell on it, more or less flexed, the sagittal suture being in one of the oblique diameters and in the axis of the brim. This may still obtain in the generally contracted pelvis. In the flat pelvis, however, the head lies in an extended position; and, if we reflect that, when in the transverse diameter, before flexion can take place, the parietal protuberances must have cleared the brim, we shall see that no other position is possible. But besides this, you will find on placing a foetal head over the brim of such a pelvis that the broadest part of the head (the biparietal diameter) is placed to one side of the promontory—where alone, in fact, it can find room which seems almost made for it. In moderate degrees of this deformity, again, the vertex of the head will be considerably below the plane of the brim; but in very severe contraction the vertex will be more nearly in the plane of the brim. The head in the flat pelvis, again, whilst having its antero-posterior diameter in the transverse diameter of the pelvis, will have its sagittal suture lying nearer to the sacrum, however, than to the pubes.

A few words here as to certain diameters of the head, and the relations of these diameters to the antero-posterior diameter of the flat pelvis, will not be inappropriate, since I find, in the writings of otherwise precise men, so much laxity in this matter, that I can only conclude either that they are not very clear on the point themselves, or that having a vague way of putting what is clear to them, it is often impossible for their readers to understand what they mean to teach.

We have two transverse diameters of the head quite distinct from each other. One of these is the coronal—the greatest dis-

tance between the two sides of the coronal suture. I prefer to call it the coronal, and not the bitemporal, the name this diameter usually receives, for when thus named it is apt, I find, to be confused with the bimastoid, with which it has really nothing whatever to do. The other of these transverse diameters is the biparietal—the distance between the two parietal protuberances. Now these diameters indicate the greatest breadths of the cranium at different portions of it, and they are about $1\frac{1}{2}$ inch apart. The measurements of these diameters *at the base*, viz., the bizygomatic and the bimastoid, are less than the corresponding diameters in the cranial vault; and the anterior pair of diameters are individually less than the corresponding diameters posteriorly. Through the kindness of Professor Cleland of this University, who made the coronal section specially for me, I am happily able to show two tracings of sections of a nine months' foetal skull, as nearly as possible through these diameters, showing the two anterior and the two posterior diameters. The measurements will be found to be as follows:—

Bizygomatic, 2 $\frac{1}{2}$.	Coronal, 2 $\frac{1}{2}$.	Increase, $\frac{1}{2}$.
Bimastoid, 2 $\frac{1}{2}$.	Biparietal, 3 $\frac{1}{2}$.	Increase, $\frac{1}{2}$.

Only one other diameter—but this time a pelvic one—must be borne in mind. As this diameter has not been recognised as such, so far as I know, though its posterior pole has often been referred to, it may simplify matters if I give it the name of *lateral conjugate* diameter of the brim. It is an antero-posterior diameter, but lies, as its name implies, to either side of the true conjugate, and not always strictly parallel to it. I find that in contracted pelvises this diameter measures *from a quarter to half an inch* more than the true conjugate. Each lateral conjugate will obviously be of the same length, unless the brim is irregularly contracted. The posterior pole of this diameter will be at the inner portion of the ala of the sacrum, and the anterior pole will be at some part of the pubic bone immediately behind its crest.

Viewing the head now in its relation to the brim of a flat pelvis, on which the vertex is resting, we are able to put the matter briefly thus: the head is extended with its long diameter in the transverse of the pelvis, its coronal diameter in the true conjugate of the pelvis, and its biparietal in the lateral conjugate of the pelvic brim. When the *base* of the cranium rests above the brim, the head is still extended, and has still its long diameter in the transverse of the pelvis; but now its bizygomatic diameter is in the true conjugate, and its bimastoid in the lateral conjugate. In the generally contracted pelvis, when the disproportion is decided, the head will have, whether vertex first or base first, substantially the same position as the above, but it will occupy equally the two sides of the pelvis. It will also have the Naegele, but not necessarily the Michaelis obliquity. If, however, the disproportion

be not great, the head can then assume a flexed position, and lie in an oblique diameter of the pelvis. I feel satisfied I am right as to these two different positions of the head in the *generally contracted pelvis*, though I am unable to name any authority in support of these views.

I know from having placed the head of a still-born child at the brim of a generally contracted pelvis, which it filled *completely*, that the head naturally took up its position and passed the brim, as in the flat pelvis, that is, extended, and in the transverse of the pelvis, whether drawn through vertex first or base first, instead of lying in the oblique diameter of the pelvis. If a foetal skull be placed at the brim of the generally contracted pelvis, which it does *not* occupy to the full extent, it will, I find, occupy an oblique diameter. Generally contracted pelves are not, however, *all necessarily strictly* "æquabiles," but incline either to the flat or the malacosteon type. And according as such pelves incline to the flat will the head lie in transverse and extended; whilst if they incline to the malacosteon type, it will lie more naturally in the oblique and flexed.

Thus far my remarks have been preliminary, and I trust I have made them sufficiently clear for you to follow me next, whilst I consider the question as to where we at present stand in the matter of treatment, and, finally, when I offer you my views as to the settlement of this most important and difficult problem.

This subject has, especially in recent years, been largely engaging the attention of obstetricians, but the writer who has done most towards attempting its practical solution is the late Sir James Y. Simpson. His papers on "Turning as an Alternative for Craniotomy and the Long Forceps in Deformity of the Brim of the Pelvis" impress me as a wonderful illustration of most elaborate and skilful pleading. Simpson's arguments powerfully influenced subsequent writers; but, notwithstanding this, their practice has, to my mind, led at least some of them to speak as if they had been "convinced against their will." The question still resolves itself into the adoption of one of these three plans,—Turning, Forceps, and Craniotomy; and the following quotations¹ will serve to indicate how far we are still from having settled the question, and how much uncertainty exists in the minds of even the highest authorities as to the proper mode of treatment.

In giving these quotations, I have been influenced by a desire to indicate what the practitioner who has no opportunity to investigate this difficult question for himself has to guide him, and to show that not only does one authority differ from another, but that the inquirer is often left in doubt as to what he is really advised to do. And here let me say that where no authority is given for any new statement I may make, it must not be inferred that I am taking, or wishing to take any credit for it, but simply

¹ These will appear as an Appendix in the February Number of this Journal.

that if brought forward previously by another I was not aware of such being the case. I thus, though intending to claim no originality, am ready to accept full responsibility for any statement made by me without quoting an authority for it. I have made no attempt at reading everything that every one before me has written on this subject, my life being too busy a one to make this possible, even if I did think it profitable.

I now come to the principal arguments used by Simpson, and will endeavour after each to give my estimate of its value as a means of ascertaining the true position of version as compared with forceps and craniotomy in contracted pelves.

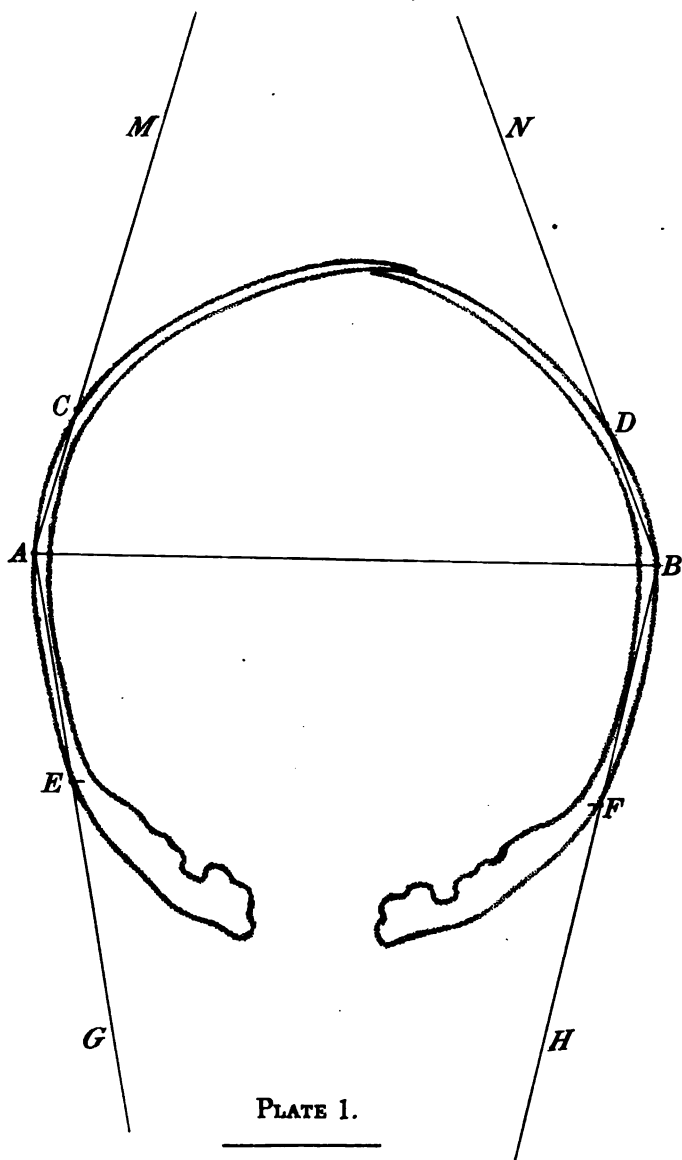
First,¹ "The foetal cranium is of a conical form, enlarging from below upwards, and when the child passes as a footling presentation, the lower and narrower part of the cone-shaped head is generally quite small enough to enter and engage in the contracted pelvic brim."

This to my mind is the great argument in favour of Simpson's position, but I am unwilling to give it the full weight which he attaches to it. It may be pointed out that the vertex is not, strictly speaking, "the basis of the cone of the head,"² but the apex of another cone, having its base the same as the lower cone, viz., the level of the parietal protuberances,—the head being thus divisible into two cones lying base to base. And, unless in exceptional cases, we find that the head, when presenting, has a not inconsiderable portion of its vertex below the level of the brim, thus giving full effect to its wedge-like form there.

A reference to the Plates, showing the coronal and parietal sections of the foetal head for the nine months' foetus, will show to what extent that part of the head above the greatest transverse diameters is of the nature of a cone as well as that below. Let AB be the biparietal diameter, which in this case measures $3\frac{1}{4}$ in.; EF the bimastoid, measuring $2\frac{1}{4}$ in.; and CD the portion of the vertex which will be caught by a conjugate diameter measuring also $2\frac{1}{4}$ in. In these Plates I have converted the portions of the cranium above and below the broadest diameters into their corresponding truncated wedges, reckoning the sides of the wedges to pass through points C, D, E, and F, where a conjugate, which would admit the rigid base, would impinge, and compression would begin. MABN and GABH, in the two Plates, represent these wedges. Now, by measuring the angles at the base of each of these four wedges, and taking the mean of each pair of angles, I find that the *mean average slope* of the upper cranial wedge (Plate I.) is $71\frac{1}{4}$ degrees, that of the lower being $78\frac{1}{4}$; and, since the "wedge-power" (here, in the sense not of power to cause expansion, but of capacity for being compressed) is directly as the size of the

¹ *Selected Obstetric Works*, p. 408.

² *Ibid.*, p. 401.

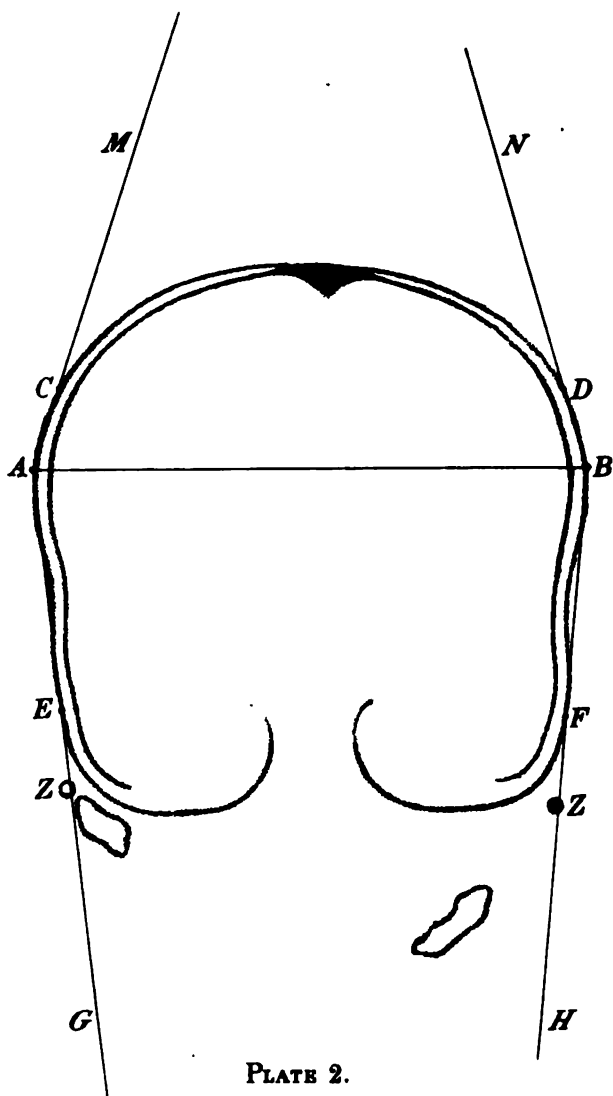


Vertical section of Cranium of nine months' fœtus through the Bi-parietal diameter; natural size.

A B—Bi-parietal diameter.

E F—Bi-mastoid diameter.

C D—Equal to Bi-mastoid diameter.



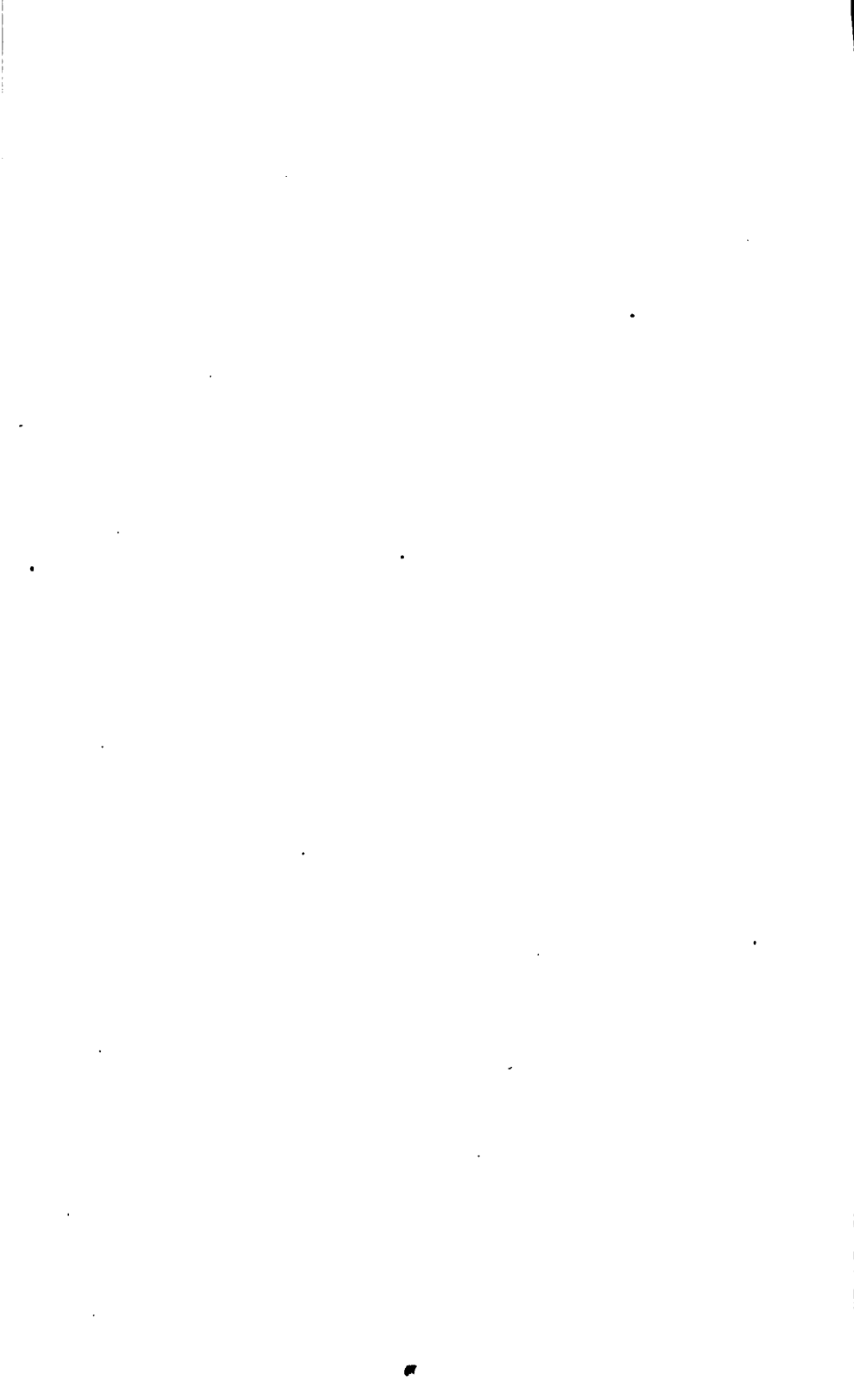
Section of same Cranium through Coronal diameter; natural size.

On the left side the Saw has cut through the root of the Zygoma, but the position the Zygomatic process would have had is indicated by a small circle.

A B—Greatest Coronal diameter.

Z Z— Bi-zygomatic diameter.

E F and *C D*—Equal to Bi-zygomatic diameter.



angles at the base of the wedge, the wedge-power of the upper is not so very much less than that of the lower wedge. In Plate II. the mean average slope of the upper wedge is $73\frac{1}{4}$ degrees, that of the lower being $84\frac{1}{4}$. The value or wedge-power of the upper is, therefore, quite comparable to that of the lower wedge. We thus see that instead of the upper part of the head being looked on as a flat surface, it differs as a cone exceedingly little from that whose truncated apex is the base of the cranium. A reference to Professor Cleland's paper¹ "On the Form of the Human Skull at Different Ages," will show that if we wish to have a child whose head will be an inverted truncated cone, as described by Simpson, at its birth, we must keep that child in utero till it has reached the age of twelve years.

Dr Hodge endeavours to reply to this argument of Simpson's also, but it is apparent that he has not hit the mark, for he speaks as if the *occiput* entered the brim first, failing to see that this is impossible in the flat pelvis.

The exceptional cases, in which the head seems to rest on the pubis rather than in the brim, and thus deprive the vertex of any benefit that it might have from its wedge-like nature, are explained by Dr James Wilson to arise, not from the conjugate being so small as to prevent entirely the descent of the apex of the upper wedge of the head, but from an abnormally great inclination of the pelvic brim, in which the sacral promontory lies nearly *over* the pubis. As I do not think that this species of pelvic brim has been recognised by obstetric operators as bearing on the question of treatment, I give Dr Wilson's² quotation from Bland:—

"Although the sacrum may project so much or advance into the pelvis so far as to reach within two or three inches of the pubis, consequently the entrance into that cavity would be only of that diameter if the bones were directly opposite to each other, yet the pubis being placed something lower than the greatest projection of the sacrum, and opposite to a part of that bone that diverges backward, the real distance between them may be much more considerable than to the touch it may seem to be; whence it happens that in cases where the projection of the sacrum has occasioned exceeding great difficulty in the beginning of the labour, opposing an almost insuperable bar to the entrance of the head of the child into the pelvis, by directing it too far over the pubis, yet when that direction has been altered by the crotchet, or by any other means, and the head brought into the line of the centre of the pelvis, the conclusion of the labour has been frequently effected with very little exertion or force."

Dr Wilson³ further remarks on this subject, "In all these deviations the labour must be affected less or more, unless the belly of the woman is pendulous, carrying the uterus so far forward as to

¹ Proc. Phil. Soc. Glasg., March 1885.

² *Glasgow Medical Journal*, 1856, p. 398.

³ *Ibid.*

make its axis correspond to the altered axis of the pelvic brim. In the higher degrees of obliquity, as in those cases referred to above, when the obliquity appeared to be not less than 80 degrees, I am fully convinced that nothing except turning can possibly save the child from its destruction by the crotchet."

Besides the influence this kind of brim will have in preventing the head from entering it, it seems to me that it will be in such cases and from such a cause that the posterior parietal bone is the presenting one.

Second,¹ "The hold which we have of the protruded body of the child, after its extremities and trunk are born, gives us the power of employing so much extractive force and traction at the engaged foetal head, as to make the elastic sides of the upper and broader portion of the cone (namely, the biparietal diameter of the cranium) become compressed, and, if necessary, indented, between the opposite parts of the contracted pelvic brim, to such a degree as to allow the transit of the entire volume of the head."

I have no objections to this proposition, with the exception of the statement that the *biparietal* diameter of the cranium becomes compressed, the fact being that the biparietal diameter is never the diameter which is compressed by the conjugate in flat pelvis. I would also add that if it is inferred that the hold in head last cases is better, and the extractive force greater than with the forceps, this can only be true if the forceps are wanting in proper length and strength. I cannot admit either that, though the hold is good, the safety of the child's neck is not, to some extent, endangered by the traction. Dr Goodell's plan, however, of applying a *vis a tergo* to the head diminishes this danger very materially.

Third,² "The lateral and very temporary compression of the foetal head by the contracted sides of the pelvis, such as we can produce and effect on artificial turning and extraction, is less dangerous to the life of the child than its oblique or longitudinal compression with the long forceps."

To this I would say that we should have some evidence as to danger from *antero-posterior* compression of the head. Is the usual compression in an ordinary case of protracted labour not largely in the *antero-posterior* diameter? Is the "sugar-loaf head" not an illustration of antero-posterior or sub-occipito-frontal compression and consequent occipito-mental elongation?

Fourth,³ "In the mechanism of head cases the neck, as is well known, becomes early flexed in the labour, so that the chin is brought towards the top of the sternum, and the vertex or upper and back portion of the head first becomes pushed downwards into the pelvic aperture, and thus constitutes the presenting part—in

¹ *Loc. cit.*, p. 408.

² *Loc. cit.*, p. 494.

³ *Loc. cit.*, p. 405.

other words, the broadest part of the cone of the whole child because the broadest part of the head or its biparietal diameter is thus naturally first driven downwards into the pelvic cavity, and is first directed against the contracted brim."

Now, the head has only to be placed at the brim of a *sufficiently contracted* pelvis to see that, *whether as vertex first or base first* the head *must* lie with its coronal or neighbouring diameter in the conjugate diameter of the pelvis, and in *an extended condition*, the parietal protuberance being to one side of the promontory. This will be found to be the case in the flat pelvis, and where the disproportion is sufficient in the generally contracted pelvis also, as has been previously pointed out. But let the head be flexed, as it sometimes is, in the generally contracted pelvis, and it will be found that the more this is done the more is the biparietal diameter brought into the line of the conjugate of the brim, although even with the greatest amount of flexion this broadest diameter in the moderate degrees of the generally contracted pelvis, as in the normal, *is never absolutely in the true pelvic conjugate, on account of the long diameter of the head lying in the oblique diameter of the pelvis, thus throwing the posterior parietal protuberance to one side of the promontory of the sacrum.* In the flat pelvis, however, and in the greater degrees of generally contracted pelvis, since the head lies in the *transverse* diameter of the pelvis, flexion would necessitate the biparietal diameter being thrown *completely* into the line of the true conjugate of the pelvis.

Dr A. R. Simpson¹ acknowledges that in the flat pelvis, with the vertex first, flexion really does not take place, but rather increased extension by descent of the forehead. But he evidently still looks on the biparietal diameter as requiring in some part of its course to pass the narrow conjugate, remarking, "The left parietal bone then rounds the promontory, with its protuberance to the left of *the most projecting part of the promontory*, and as it is squeezed past this point, it may receive an indentation towards its anterior extremity."

Lusk, who quotes largely from Litzmann and Michaelis, on this point falls into the same error; for, after describing the partial descent of the sinciput, he adds,² "by the time, however, the bitemporal diameter becomes fairly fixed in the conjugate, the anterior fontanelle moves upward and forward towards the side-wall of the pelvis, while the small fontanelle sinks downward, and occupies a position near the centre of the cavity. This movement is not simply a crowding of the entire head in the direction of the brow, but is due to a rotation of the head upon an axis furnished by the conjugate diameter, the symphysis and the promontory furnishing the pivotal points. By the time, in the rotation of the head upon its fronto-occipital diameter, the posterior

¹ *Contributions to Obstetrics and Gynecology*, p. 170.

² *Science and Art of Midwifery*, p. 452.

boss reaches the level of the promontory, the largest circumference of the child's head has already engaged in the straitened brim, and the influence of the pelvic flattening upon the mechanism of labour ceases." Here we have, after descent of the brow, a movement of flexion in bringing the posterior parietal boss towards the middle line, and to the level of the promontory. His explanation as to how the parietal boss gets past the promontory is, that already the greatest circumference of the child's head has passed the brim by the previous descent of the anterior parietal boss, and therefore there is no difficulty now in the posterior parietal protuberance clearing the promontory. He *may* mean that the biparietal diameter passes in the *lateral conjugate*—that is, entirely out of the influence of the promontory—but he does not make this plain, if he even thinks of it as so passing into the cavity.

Nor is it necessary to infer what Dr Lusk means on this point; for, that he considers the parietal boss to require in the flat pelvis, at some part of its transit through the narrow brim, to be forced round the promontory, and not at its side, is made certain at page 457, where, indeed, he gives an illustration, borrowed from Dohrn, of a foetal cranium, which in passing through the brim of a "simple flattened pelvis" had been marked by the sacral promontory—this mark in one part of its course being actually on the apex of the parietal protuberance. Now, I am willing to admit that this may not be an imaginary case; but what is its significance if true to nature? Simply this, that the promontory which had marked that cranium *did not belong to a flat pelvis*. And that I am justified in making this assertion, a reference to page 479 of *German Clinical Lectures*¹ will prove; for, in an article there by Prof. Dohrn, of Marburg, on "Artificial Premature Labour with a Narrow Pelvis," we have the measurements of "a simply flat pelvis" given, in which the I.S. distance is *less*, and the I.C. distance *more* than in the normal pelvis,—evidently, therefore, neither a generally contracted pelvis nor a flat pelvis, whatever else it may have been. Here, then, we have the error Sir James Simpson committed in 1847 repeated by a distinguished German professor in recent years, and adopted in 1882 by one of the most distinguished of American obstetricians, and one who has laid special stress on this very subject.

Though I have not the support of Dohrn in my views as to the mechanism of labour in the flat pelvis, I am glad to find that our experience has brought us to the same conclusion, for I find him at page 483 saying, "If I appeal to my own" experience, "it speaks decidedly in favour of head presentations."

Charpentier² also accepts this illustration of Dohrn's as evidence of the necessity for the parietal boss being compressed by the sacral promontory in the *flat pelvis*.

¹ New Sydenham Society, 1877.

² *Traité Pratique des Accouchements*, p. 180, vol. ii.

Sir James Simpson's argument, therefore, amounts to this, that if the forceps are applied so as to bring the vertex first through the brim of the contracted pelvis, flexion will necessarily take place, and the biparietal and not the bitemporal (coronal) diameter will have to be squeezed through the narrow conjugate. We have just seen that if the head is in the oblique diameter of the pelvis, this is a *physical impossibility*. That theoretically it *may* happen when the head is in the transverse cannot be denied. If it did ever happen, I should say that Nature had taken the most awkward way to get out of her difficulty. That it *actually does not* happen the following cases will at least help to prove. See also, in support of my contention, Spiegelberg's¹ and, in part, Dohrn's views as quoted by Charpentier.

The first case I shall give in detail,² though only the last portion of it bears on this point. The rest of it, however, will be referred to further on. I published this case in part in the *Glasgow Medical Journal*.

CASE I.—Mrs M'L. (abstract)—*First* child, breech, still-born. *Second*, forceps, still-born. *Third*, forceps, child alive. *Fourth*, forceps, child alive. *Fifth*, forceps, child alive; head indented, left frontal bone; generally contracted pelvis.

I have this day (11th November 1885) seen these two children. They are a strikingly healthy and bright-looking pair. I found the mark on the girl's head quite distinct to the touch, as a spoon-shaped depression about $1\frac{1}{4}$ inch broad; but on account of a good crop of hair, the mark could not be seen.

See also Cases V. and X.

For the next case I am indebted to Dr G. of Glasgow.

CASE II.—Mrs S. (abstract)—*First*, premature, child alive. *Second*, miscarriage. *Third*, forceps, still-born, deep indentation over junction of frontal and parietal bones of one side. *Fourth*, forceps, child alive, deep indentation at junction of left frontal and parietal bones. *Fifth*, forceps, child alive, head indented on left frontal bone. *Sixth*, child dead, head marked as in others.

Now, if we note what part of the head in these cases of delivery *with the forceps* is indented by the sacral promontory, we will find it to be the bitemporal always; and this region is at least $1\frac{1}{4}$ inch anterior to the parietal protuberances. We thus see further that this argument is fallacious.

Fifth,³ "That the *duration* of the efforts and sufferings of the mother is greatly abridged by turning, when used as an alternative for craniotomy and the long forceps, and that thereby her chances of recovery and safety are increased, whilst the infantile mortality

¹ *Traité Pratique des Accouchements*, p. 164, vol. ii.

² These cases will be fully detailed in the February Number of this Journal.

³ *Loc. cit.*, p. 409.

is decreased, because this operation can be performed earlier in labour, and more speedily than the application of the long forceps or craniotomy."

This is obviously an argument not against the forceps, but against *delay* in the use of the forceps, for this operation can be performed, if necessary, quite as early in labour as turning can. If a little more dilatation of the os is required for the introduction of the forceps, a very few minutes, with one of Barnes's bags, would suffice to accomplish this. The same remarks may be applied to the case of craniotomy, for this is rather an argument not against craniotomy, but against unwarrantable delay in the performance of the operation. But is danger to the mother and the infant decreased by the frequent practice of turning? Not necessarily so, even should it effect an abridgement of the labour. For although it is true that beyond a certain limit as to time, labour which has become what we call prolonged is dangerous to both mother and child, yet within this limited time which we without fear permit, because we look on it as normal, it is *not* true that a precipitate labour is safer than one of average length. Interference in the course of labour with the sole object of shortening its duration, if this had not already been, or was not threatening to become, prolonged, used to be called "meddlesome midwifery;" here, however, we have it called the avoidance of "dangerous delay." The fact that the labour can be terminated more speedily when version has been determined on than if the forceps were to be used "cuts both ways;" for whilst it is true that the time required is shorter, it is no less true that there is vastly greater need for hurry in the case of version than in the case of the forceps. For whereas five or six minutes may be taken as the maximum time the operation must last from the instant the funis has passed the os uteri, if the child is to be saved, the head can in the grasp of the forceps, as has been pointed out by Martin, be moulded for one hour at least without the slightest danger to the child. The relative effect of these operations on the mother I shall consider later on.

Sixth,¹ Another position taken up by Simpson in support of his contentions, and on which he dwells very fully and with no little satisfaction, is that numerous cases have occurred in which a woman has been delivered of dead, or nearly dead children by forceps, and in previous or subsequent labours of living and sometimes vigorous children by version, or when the breech had been originally the presenting part.

Now this would be a very powerful argument if exactly the reverse did not frequently happen. I am not aware that many instances of this have been recorded; but I have met with many myself, and I have no doubt that many

¹ *Loc. cit.*, p. 397.

unrecorded cases have happened in the practice of others. As I consider it important to demolish this argument, I select a few of these cases from my notes, remarking beforehand that Case I., that of Mrs M'L., is a case in point. Further, had I, in this case, turned at the fourth confinement after failure with the forceps, turning would have got the credit for the "very large boy," who "cried vigorously when born."

CASE III.—Mrs M'D. (abstract)—*First*, breech, still-born. *Second*, vertex, child alive. *Third*, vertex, forceps applied to antero-posterior diameter of head, child alive. These three labours all attended by myself.

CASE IV.—Mrs P. (abstract)—Ten children; nine vertex, alive, two of these forceps; tenth child, breech, still-born.

The next case came under my notice through the request of Dr R. of Glasgow, that I should see her with him, in order that we might decide as to the induction of premature labour. It was a case of generally contracted pelvis, the true conjugate measuring about $3\frac{1}{2}$ inches.

CASE V.—Mrs C. (abstract)—*First*, forceps, still-born. *Second*, forceps, child alive. *Third*, without help, alive. *Fourth*, forceps, child dead, spoon-shaped depression near ant. fontan. *Fifth*, labour induced at eight months, no assistance, child lived three days. *Sixth*, full period, vertex, child alive, short second stage, no assistance.

In this case it is evident that if the breech had presented in the last labour, it would naturally have been looked on as a case in proof of the advantage of version.

The notes of the following cases have also been put in my possession by Dr G., and I append his own remarks on the case:—

CASE VI.—Mrs J. (abstract)—*First*, forceps, child dead. *Second*, *third*, and *fourth*, forceps, children alive. *Fifth*, hand presentation; version, delivery of head extremely difficult; head born ten minutes after body; child nearly dead, probably less in size than third or fourth.

"Would, in view of above experience, prefer much to use Simpson's long forceps for delivery than turn, since in the last case the child was very nearly sacrificed."

In the next case both the labours occurred in the Hospital.

CASE VII.—Mrs L. (abstract)—Full time, vertex, axis-traction forceps; a large child, still-born; mother good recovery. Following year labour induced at $7\frac{3}{4}$ months, conjugate $2\frac{3}{4}$ inches; Barnes's bags dilated as to a circle of about 9 inches circumference;

footling presentation, great difficulty with extraction of head; child still-born, weighed 4 lb. 6 oz.; hæmorrhage from laceration of cervix; pulse rose to 180.

I confess that the results of these two labours impressed me very strongly; the fact of a large child having been extracted, although dead, with no damage to the mother, made me feel assured that had the second soft and immature head presented at the brim, I should have had, with the forceps, no difficulty in extracting it without risk to the infant, and, I *now* think, without the slightest laceration of the cervix.

CASE VIII.—Mary R. (abstract)—Hospital, second pregnancy. *First* had been vertex, child alive. *Present* labour footling; head delivered by forceps 7 minutes after body, child with difficulty brought round.

The next case was seen by me in the country about two years ago. I found the child's neck had parted during traction, and that the head was still in the uterus. I applied Simpson's long forceps, using powerful compression, and delivered the head in about ten minutes. I give verbatim the letter from Dr W., who was called in as a consultant before I was summoned.

CASE IX.—Mrs W. (abstract)—Six children in all. *First*, forceps, child still-born. *Second*, vertex, attended by a woman, child alive. *Third* and *fourth*, forceps, still-born. *Fifth*, vertex, forceps, afterwards version; traction separated head from body. *Sixth*, vertex, attended by a woman; child alive, slightly premature.

The following is a case of indentation of the head after an unaided delivery, showing how easily such a depression can be made in a slightly premature child. The flattening of the vault probably arises from the fact that there was not time for moulding on account of the early yielding of the frontal bone:—

CASE X.—Mrs L. (abstract)—Hospital case. Third confinement, previous labours forceps, this one unaided; child alive, considerable depression of right frontal bone.

I have, in discussing this sixth argument of Simpson's, dwelt only on the relations of turning to the forceps, but his reason for preferring turning to craniotomy can be easily understood when we find him speaking of the latter operation in these terms:—"The operation is not one which is either morally or professionally justifiable, if the child be still living." It will be seen that the safety of the mother is here forgotten in the determination not to destroy the child, but give it time to die.

But there are other objections to the position taken up by Simpson and his followers, that, as a general rule, failing to effect

delivery with the forceps, turning and not craniotomy should be resorted to: First, that it does not follow that turning will be speedier and safer than craniotomy, and that if craniotomy has after all to be performed, the woman is certainly, *as a rule*, in a worse case than if the child had been at once sacrificed. If the woman is seriously damaged by this attempt to save the child, we ought to feel but poor consolation from the reflection which Barnes seems to think sufficient, that we did our best for the child.

Again, as a further objection, let us bear in mind that in normal pelves the risk of laceration to the cervix and vagina from turning is very much less than from the same operation in contracted pelves, and I am not aware that this has been dwelt on as germane to the discussion, if it has even been at all noted by writers on the subject. The reason for the increase of this danger in contracted pelves lies in the fact, that whereas in turning with a normal pelvis nearly the whole of the time spent in extracting the head through the pelvis can be expended in dilating the cervix and vagina (though I fear that in actual practice this is too often forgotten even by the most careful of us, on account of the great importance of the bony canal in the mechanism of labour), on the other hand, every moment of the time at our disposal is required in the contracted brim to drag the head through it, and we are compelled, if the very object with which we undertook the operation is to be successfully carried out, to "hasten," not "slowly," but with all despatch, the transit of the head. That this has too often been attended with disaster to the maternal soft parts I am prepared to prove. Indeed, the case of Mrs L., in whom labour was induced, and who suffered to an alarming extent from hæmorrhage after labour, is a case in point. Another instance of this will be found reported in full (though for altogether a different reason) in the *Edinburgh Obstetrical Transactions*, vol. vi. This case happened in the Maternity Hospital, Glasgow, while I was on duty, and the woman was delivered by Dr Wm. Turner, then house-surgeon, and one of the most intelligent and careful of house-surgeons. The cord had prolapsed, the foot being the presenting part, and though Dr T. recognised the case as one of pelvic contraction, there was no evidence to satisfy him of contraction so severe as was subsequently found to be the case.

I give the case briefly, but as it is, I think, the only instance of a living child having been extracted through a pelvis with a true conjugate diameter virtually under $2\frac{1}{2}$ inches, it will repay a reference to the full report.

CASE XI.—M. C. (abstract)—Hospital case, full term, footling and funis, head detained four minutes at brim, child alive, cervix torn at both sides; woman died; justo-major pelvis flattened.

In referring to this case I do it, not to show that the cervix *may* be lacerated under these circumstances, but to emphasize the

fact that, as happened in my own hands in the case of Mrs L., the desperate efforts which a conscientious man makes in such exciting circumstances to save the life of the infant make the accident a most natural result. But it may be said that if care is taken, and the os is sufficiently dilated to permit of turning, need this accident occur? Is it really the case that much if any additional expansion of the cervix is required, after danger to the child begins—that is, as soon as the umbilicus has cleared the cervical orifice? I say it is, and to an extent to which not sufficient weight is given in books on midwifery, whilst, indeed, the subject is ignored as if of no consequence in most works. Since my attention has been directed to this subject, I have noticed many similar instances of this accident, and as I consider it of very great importance in the settlement of this question as to version, you will excuse me going a little more into detail on this point. I have ascertained that the largest of Barnes's bags can be expanded by water till it reaches a circumference of $9\frac{1}{2}$ inches before bursting. And here let me add that as the life of india-rubber is a short one, these bags are not safe for use if more than a very few years old.

Now, from measurements I have made, or have had made for me by careful observers, of children at or very soon after birth, I find the circumference of the various parts of the child's body, with reference to its passage through the soft parts of the mother, to be as follows on an average of eleven cases:—

Pelvis, . . .	10·18 inches.	Half-shoulder, . . .	13 inches.
Half-Breech, . .	11·54 "	Sub-occip.-ment., . .	10·36 "
Breech, . . .	13·75 "	Sub-occip.-front, . .	12·61 "
Thorax, . . .	11·68 "	Sub-occip.-bregmat., .	12·18 "

These measurements I have seen no similar circumferences to compare with, except Dr Matthews Duncan's head girths. The last two of my list he gives as respectively 12·75 and 12·5, nearly the same relatively as mine, though greater absolutely. He finds the sub-occip.-vert. to be 1·5 less than the sub-occip.-front. What must strike one at a glance here is the fact that the breech is, on the average, the greatest of the circumferences. I suspect, however, that when we consider the extent to which the breech could be squeezed in its passage through the strong cervix, compared to the less yielding structures of the head, we must make some allowance for this measurement, though I do not think it, if of the average size, could be so reduced more than an inch. This would bring it, when so compressed, to almost exactly that of the sub-occipito-frontal, which is still, I think it must be acknowledged, more than would have been believed. It is easy now to see how in a case of podalic version, and where five or six minutes only can, if the child is to be saved, be allowed to elapse from the time the os encircles the belly of the child, and thus compresses the funis, till the birth of the head, and how, if nearly the whole of

this time is required for the mere transit of the head through the pelvic brim, almost no time is left for the expansion of the cervix from 10 inches—the girth of the child's pelvis—to 12½ inches, the greatest girth of the head. So, without having these measurements forcibly impressed on us, how can the soft parts be expected to escape? Let us bear in mind the fact, that it is just as the perinæum is sweeping over the sub-occipito-frontal region, that tears in this region occur or begin, and that, though we can see the process, and to a great extent control it, we cannot always prevent it. It will not then surprise us that so sudden a transition, viz., from the pelvic girth to that of the sub-occipito-frontal, should do serious damage to the cervix, since less rapid transition, viz., from the sub-occipital-vertical to the sub-occipito-frontal, makes so much difference in the case of the perinæum. But it may be said that in normal cranial presentations this laceration of the cervix may take place also, and, indeed, is known to take place, to some extent at least, in almost all cases of normal first labours. True, but with what admirable mechanism is the cervix dilated in cranial presentations, when we think of the steady and alternate expansion and relaxation of the os during the first stage, fortunately a stage that, as a rule, is not interfered with by “meddlesome midwifery.” I have seen in cases of head last labour, that the cervix was so tightly embracing the shoulders that the cause of the difficulty in bringing them down was not the bony contraction at the brim, but the tight and undilated cervix.¹

This may be a fitting opportunity for noting that Simpson, in arguing in favour of turning, does indeed refer to the danger of laceration of the uterus, in order to prove that this injury is “*somewhat less likely*” to arise from turning than from forceps or craniotomy, but a perusal of his argument will show that he thinks only of *protracted bruising* as a cause of laceration, and does not suspect that version, by causing, as I have shown, a too rapid expansion of the cervix, may cause laceration of that part of the uterus. He does speak of version as to be performed after the dilatation or dilatability of the passage will allow, but this is not referred to as a caution on account of danger from the operation, but as indicating how soon version may be attempted. Nor is it only a laceration of the cervix that may result from version in contracted pelvis, for Dr Radford² mentions a case where, although it is stated that the os had become softened, the tear had evidently begun at the os during the process of extraction, and had extended

¹ The following case has come into hospital since paper was read (abstract)—Second confinement. First eleven years ago, full time, vertex, labour normal, child alive. Present labour, membranes ruptured five days before admission, presentation transverse, right foot brought down; labour allowed to proceed naturally; five minutes occupied in passage of head, which was caught by tight cervix; child small, heart beating; died.

² *Cases of Laceration of the Uterus*, p. 17.

afterwards through the cervix and part of the body of the uterus. But I can appeal, in further proof of the danger of laceration of the cervix from rapid dilatation, to Emmet, the great American gynecologist,¹ for, without any special reference to version, but only referring to rapid labours in general, he says, "I cannot divest myself of the conviction that rapid labour will be found, on further observation, to be a far more important factor in causing this lesion than has been indicated by this record."

Perhaps it will be profitable at this stage of our inquiry to ask you to consider with me to what my remarks, so far as they have gone, have led up. What now are we to think of Sir James Simpson's arguments in favour of version? What, if any, portion of them are we to accept? what are we to consider as doubtful, and what are we to reject as erroneous? And having settled this, what further is to be our practice in the future, in cases of delayed labour arising from obstruction at the conjugate of the pelvic brim?

We have seen that Simpson's propositions may be reduced to six, these embodying the substance of his conclusions in favour of version as an alternative for craniotomy and the long forceps. I shall tabulate them as queries, and give a brief reply to each, and then ask you to decide between us.

First, Is the foetal cranium of a conical form, enlarging from below upwards, the vertex being the basis of this cone?

This is true to *so slight an extent* as to make it *almost a distortion of the truth*.

Second, Does the hold which we have of the protruded body of the child, after its extremities and trunk are born, give us the power of applying so much extractive force and traction at the engaged foetal head as to make it become compressed to a greater degree than would the forceps if the vertex presented?

Yes, by fixing the head in the brim, and thus being compelled to pull the head in the axis of the brim.

Third, Is the lateral and temporary compression of the foetal head by the contracted sides of the pelvis, as after version, less dangerous to the life of the child than its oblique or antero-posterior compression with the forceps?

This is very doubtful, and at anyrate has, if true, never been proved.

Fourth, In the flat pelvis, in head-first cases, is the head necessarily flexed, so as to bring the biparietal diameter into the true conjugate of the brim, instead of, as after version, to one side of it?

No; in the strictly flat or the generally contracted pelvis, never.

Fifth, Is the duration of labour shortened by version as com-

¹ *The Principles and Practice of Gynecology*, p. 450.

pared to forceps, and are the chances of the mother and child thereby increased?

The labour is shortened, but the chances of the mother and child are often thereby *decreased*.

Sixth, Have women frequently been delivered of living children by version or breech presentations, who at previous or subsequent labours were delivered of dead children by forceps?

Yes, and women have frequently been delivered of living children by forceps, or in unaided labours, who at previous or subsequent labours were delivered of dead children when the breech presented.

This much is granted, then, that a head coming base first can pass through a contracted brim in less time than one coming vertex first, and that, when so extracted, the child may be alive. If, therefore, this operation can be performed with little or no risk to the mother, it follows that turning will, under circumstances unfavourable to the forceps, be sometimes a permissible operation. The degree of contraction of the conjugate, which will make version under any circumstances justifiable, it is important to determine; but instead of arguing the question on theoretical grounds, it will be better to speak from experience. Case XI. shows that this is *possible* with a conjugate of $2\frac{3}{4}$ ins.; and I myself have extracted a child weighing 7 lbs. through a conjugate of $2\frac{1}{2}$ ins. The child gasped but did not survive. I regret that I am able to state exactly the size of the conjugate, for the woman died from peritonitis, and careful measurements of the pelvis were made at the post-mortem. In these two cases, Dr Turner's and my own, the pelves were very roomy transversely. Could forceps bring a child of average size through such a conjugate? I am unable to say; but a reference to Case VII. will show what axis-traction forceps have done. Though this child was still-born, it is not to be forgotten that it was a large child, and that the conjugate in all probability measured only $2\frac{3}{4}$ ins.,—so small, indeed, as to prevent a premature child, weighing 4 lbs. 6 oz., from being born alive, when presenting as a footling. A common objection to the forceps in these cases of *flat* pelvis, for it is here mainly that forceps are apt to be laid aside for version, is that, in the words of Dr A. R. Simpson, "such a grasp of the head," viz., in its antero-posterior diameter, "would produce, on the one hand, a dangerous compression of the child; and on the other, would at once lessen the size of the foetal head in the roomy transverse diameter of the pelvis, and tend to make it bulge in the direction of the contracted conjugate."¹

Now, not only do the forceps grasp the head in its oblique diameter, *as a rule*, and thus actually assist the lateral compression instead of hindering it, but compression is *hardly at all necessary* if the forceps are skilfully handled. The fact that the child is

¹ *Contributions to Obstetrics and Gynecology*, p. 170.

born at all, under these circumstances, proves that the head was not really bulged in the direction of the contracted conjugate, or its birth would have been prevented, or at least hindered, instead of, as several of my cases reported here prove, really accelerated—women having had living children by forceps and dead children by presentation of the breech. See especially Case III., that of Mrs M'D., where the grasp of the head was actually antero-posterior. The application of straight forceps to the transverse diameter of the head, *and in the lateral conjugate of the brim*, would be theoretically preferable to the oblique grasp; but I am not aware that any instrument has been devised which could do this.

But, in my opinion, the chief reason for version being by some preferred to forceps is, that in extracting the head after version the operator has no difficulty in pulling in the proper axis, for the head adapts itself to the best advantage at the brim when base first, whereas considerable skill is necessary to manipulate the vertex at a contracted brim when in the grasp of the forceps. Since the introduction of the axis-traction forceps, an instance is mentioned by Dr Simpson in which axis-traction forceps succeeded in delivering a case of *justo-minor* pelvis in which at a previous labour the child had been turned and perforation required afterwards. I do not mention axis-traction forceps because I think them absolutely necessary. What I insist on is, that the traction should be axis-traction. Now, in my experience axis-traction can be performed by the ordinary forceps in the hands of a skilful man, whilst in the hands of an unskilful practitioner axis-traction forceps are likely (I speak from observation) to be used for non-axis-traction.

But given a case where the forceps have been used and fairly tried for one hour, without being able to engage the head in the brim, are we now to turn or perforate? This is the point of greatest difficulty to determine. In my experience it has been too much in the past a routine practice to decide that because the forceps have failed the child must therefore be turned; and I believe that this is often the practice even although the child is believed to be dead, under the impression that version is a safer operation to the mother than craniotomy. Now, let us consider what we have to settle before determining as to whether in such cases we should turn or perforate. First, as to the child. By the former operation the child has a chance of life; by the latter operation the child is sacrificed. Therefore, were we to consider the matter only from the point of view of the safety of the child, we should in all such cases, if the child be alive, turn. But what about the mother? It may be taken for granted that craniotomy *in such a degree of contraction of the pelvis as we are at present considering* is attended with little danger to the mother, especially if the pelvis be shallow.

What is the amount of danger in turning?

Dr Barnes, in his recent excellent work, mentions that it would be dangerous under such circumstances to turn if the uterus be

firmly contracted round the child, "balling it;" or if the head is impacted or very firmly set in the brim of the pelvis; or if there is marked exhaustion or prostration of the mother. Few careful men would venture to turn under such circumstances. But take a case where there is not *very firm* compression of the child by the uterus, and not *much* exhaustion of the mother. Are we now justified in turning? We have seen that turning will give the child a chance. We know that under these circumstances turning will add some risk to the mother; and if turning results in a dead child, and craniotomy has to be performed afterwards, we know that the danger to the mother has been materially increased by the attempt to save the child. I have seen several such cases, and the lessons to be learnt from them are so valuable that I insert here three of them.

CASE XII.—Mrs M'G. (abstract)—*First three* children, labours normal. *Next three*, forceps. *Seventh*, turning after forceps. *Eighth*, forceps; all children born alive. *Present* labour, forceps failed, turning unsuccessfully attempted; woman then brought into hospital, very weak; forceps failed, turning effected with difficulty; child still-born, mother died.

CASE XIII.—Mrs G. (abstract)—*First two*, forceps, still-born. *Present* labour, forceps failed, turning unsuccessfully attempted; woman then sent to hospital; pulse 150, patient sinking rapidly; perforation and delivery with forceps in ten minutes, woman died in one hour. *P.M.* revealed laceration of vagina in Douglas's pouch. Conjugate 3 inches.

The next case occurred in a neighbouring county town. I was sent for to perform craniotomy. Though it was evident the woman could not recover, the doctor in charge—one of the most careful and skilful of country practitioners—was naturally anxious she should not die undelivered.

CASE XIV.— — (abstract)—Nine children. Forceps for first only. *Present* labour, forceps failed, turning partially effected, craniotomy consequently very difficult, woman died.

Now, what are our relative duties to mother and child under such circumstances? Are we justified in exposing the mother to serious risk in order to attempt to save the child's life? Barnes says we have done our best. Ought this to satisfy us?

Smellie¹ says, "We ought never to endanger the life of the mother to save the child." M'Lintock, the editor of Smellie's Works, adds,² "We are justified in endangering the life of the mother, if by so doing we can give to the fetus a greatly increased chance." Napoleon, when the question was put to him, is said to have replied, "Save the mother, it is her right."

At a consultation at the Maternity here, the question of version

¹ Smellie's *Midwifery*, by M'Lintock, p. 334, vol. ii. (see also p. 131, vol. iii.)

² *Ibid.*, p. 335, vol. ii.

or craniotomy was under consideration. One of the gentlemen stated the case for version thus:—

"The child is pure, innocent, and untried. The mother is rickety, has had her day, and has made a bad use of it, therefore let the child have its chance." *This doctor was an unmarried man.*

Is it not the case that we have contracted with the mother to deliver her out of her present difficulty. She has placed her life in our hands. Is it fair to risk it without her consent, even with the certainty of saving the child—especially if, as in the kind of pelvis we are considering, at a future pregnancy induction of premature labour would be probably safe to both mother and child. Though we may conclude, however, that we have no right to risk the life of the mother, yet if we feel that the operation we decide on will only risk her future health, we may be justified in attempting to save the child. In cases of doubt, I would advise that a gentle attempt at turning should be made, but that if we encounter any difficulty in the operation, perforation should be at once resorted to.

I now add a series of propositions for guidance in cases of labour obstructed at the pelvic brim. I do not offer them as final, but hope they may have a share in helping us to reach the time when the mist surrounding this important subject shall have vanished, and we shall be able to do more than we now can for the safety of both mother and infant.

In concluding, let me remind you that I have not attempted in this paper to give you a dissertation on this subject, but only a contribution to it. The present uncertainty as to treatment in cases of contracted pelvis will, I hope, be considered a sufficient excuse for venturing to do even this.

I. That *MERE disproportion between the child's head and the brim of the pelvis* is NEVER a sufficient reason for preferring version to the forceps as an original choice in the combined interests of mother and child.

II. That cases sometimes occur in which, *for other reasons*, version is to be preferred to the forceps as an original choice, but that if the child be of presumably average size, this operation should not be attempted with a conjugate diameter under $2\frac{1}{2}$ inches, and, with such a diameter, only if it is a *justo-major* pelvis flattened.

III. That the following are some of the "other reasons" for preferring version to the forceps as an original choice:—The occiput to the wrong side of an irregularly contracted pelvis; occipito-posterior position in a generally contracted pelvis, which position cannot be rectified manually (or rather bimanually); prolapse of the funis; placenta prævia; face presentation; displacement or increase in bulk of the presenting part as by the partial or complete descent of a hand or foot along with the head; great inclination of the pelvic brim throwing the head on to the pubes instead of permitting it to

be over the brim; *great* difficulty in applying the forceps, or a very tight and incomplete locking of the forceps after some difficulty in their application.

IV. That where the forceps for "other reasons" is unsuitable as an original choice, version may be tried, *not simply in the flat, but in the generally contracted pelvis also*, flexion of the head being no contra-indication.

V. That if version is decided on, the breech of the child, where this is at all practicable, should be allowed naturally to dilate the cervix; and that, if one leg must be brought down, the other should be left to increase, with the pelvis of the child, the expansion of the cervix.

VI. That if version is decided on as an original operation, it ought, if possible, to be done by the bipolar method, and as soon as the os is sufficiently dilated to permit of it—the membranes being, if practicable, kept entire after version, but ruptured at once, if this is necessary in order to keep the breech in its new position.

VII. That in cases of doubt forceps should be preferred to version as an original choice. But should the pelvis be shallow, version has this advantage, that if the body be born, the child can sometimes be made to breathe though the head is at the brim. Craniotomy will also then be less difficult to perform, should this operation be afterwards required.

VIII. That in cases in which the forceps has failed there should be some reason for suspecting other causes than disproportion (see Prop. III.) before version is attempted as an alternative to craniotomy.

IX. That the employment of version as an alternative to craniotomy, as a routine practice, is terribly hazardous to the mother, although it probably sometimes saves the child's life.

X. That in a generally contracted flat pelvis, if the child be of average size, and the degree of contraction be at all great, version is entirely inapplicable. A *short* trial should be made with the forceps. If no progress be made, craniotomy should be performed at once.

VI.—CASE OF CONGENITAL HERNIA REDUCED *EN BLOC*.

Part of a Clinical Lecture delivered by Mr JOSEPH BELL, 9th November 1885.

SINCE our last clinical lecture some of you have had an opportunity of seeing an operation which must have been very instructive. I will give you the case as briefly as possible, without missing any of the important points. The patient, a young man, æt. 19, was brought into Hospital on Friday (6th Nov.), with a history that he had a hernia; that that hernia had fourteen days before become strangulated; that it had been at once reduced, but that he had never ceased vomiting; that the vomiting had latterly become

stercoraceous or *faecal*, and he had had no passage from his bowels. When I first saw him, before hearing his history, I felt that we had to deal with a severe abdominal case. His face had the peculiar expression, well known to ovarian surgeons, that is to be seen in sufferers from advanced abdominal mischief. But in addition he had a bluish or purplish colour that was very important. It was evident that the lad was near his end, and that his only chance was immediate operation. In this I had the advantage of the kind assistance of Messrs Duncan and Cathcart. As to diagnosis, we were agreed that the lad had an undescended testicle, and that this testicle in its efforts to come down had got engaged with a hernia which had become strangulated, and on taxis being applied had unfortunately been reduced *en bloc*. The reduction having failed, it was evident that something further must be done. What ought we to do? was then the question. I have not unfrequently, in recent cases of this kind, where reduction has taken place *en bloc*, cut down where the hernia had been, followed it out, pulled it down, divided the constriction, and returned the bowel. But in this instance we knew that the bowel had been constricted for fourteen days, and I felt that the only chance the patient had might be lost by beginning in that situation, because, first, the hernia might not be found there, and second, even if it were, the bowel was pretty certain to be gangrenous, and perhaps ulcerated, so that before we got it down there was very great risk that the contents might escape into the peritoneal cavity. With the approval of my colleagues I therefore first opened into the abdomen in the middle line. I then passed my hand from the wound to the place where the hernia had come down, and found that a small coil of intestine had been caught in a tight ring with extreme firmness. We were for a moment gratified by finding that there was not a great amount of peritonitis—that it had not gone on to the stage of effusion. While Mr Cathcart kept the hernia in position, I cut down upon it from the outside in the line of the usual operation for hernia, and found lying at the internal ring an undeveloped testicle, and beside it a small kink of bowel, which was dark but not apparently gangrenous. We could, however, feel that the part where the constriction was, was very tense. To avoid the risk of the bowel giving way I passed one hand through the laparotomy wound, and with the forefinger of the other gently distending the inguinal ring, I brought the bowel safely outside the abdomen. The moment I unloosed my hold of it a quantity of *faeces* escaped, but none were allowed to enter the peritoneal cavity. Our next duty was to consider what should be done with the ulcerated bowel—whether to make an artificial anus or perform an enterectomy. Had I seen the patient a day or two after the accident I should have done the latter, but the time necessary for its performance was a strong objection. Not only was the patient's general condition very serious, but the way in which he took the anæsthetic gave us considerable anxiety. Several times during the operation we had to stop either to raise his limbs or to perform artificial respiration. I have not

the slightest doubt that the peculiar colour of his face indicated that there had been some coagulation beginning in the great vessels. It was evident that the time necessary for an enterectomy would seriously diminish his very slight chance of life. The bowel was therefore stitched to the laparotomy wound, making an artificial anus in the middle line. The undeveloped testicle was next removed, the ring stitched with catgut, the sutures being brought down to act as a drain. He was then got to bed and after a little showed symptoms of rallying, but did not live out the twenty-four hours, dying about 4.30 A.M. The two practical points I should like to impress upon you in connexion with this case are—(1.) If you have a congenital hernia of this kind, with an undescended testicle, regard it as a case of extreme gravity. Be especially careful not to reduce it *en masse*. (2.) If such an accident should happen, be exceedingly careful to have a consultation, or operate immediately upon it yourself, either by cutting down in the middle line, or by following out the hernia from its old position in the inguinal ring.

[NOTE.—In consequence of the non-receipt of Dr Crichton Brown's MS., his paper, *Some Neuropathical Studies*, is delayed.]

Part Second.

REVIEWS.

Comparative Physiology and Psychology: a Discussion of the Evolution and Relations of the Mind and Body of Man and Animals.
By S. V. CLEVENGER, M.D. Chicago: 1885.

WE are not going to deny that Dr Clevenger's studies have fitted him to approach the difficult subject which he attempts; but it might be perhaps suspected that some of the questions to which he gives an answer are beyond our present knowledge, or perhaps beyond all future human knowledge. So great, however, is his confidence in the power of science to solve all enigmas, that our author will not grant an unknowable; "for to set limits to what may be disclosed to us in the future argues a knowledge of where science will always fail which no one possesses." He starts with an admission of *ignoramus* as to ultimates; "but by what right," he asks, "can we cry *ignorabimus*?" In truth, Dr Clevenger gives an explanation of most problems which he touches. Where ideas fail words come in; and if we grant that his explanations are satisfactory, he has much diminished the realm of *ignoramus*, and his forthcoming work, to be entitled *Psychology*, if continued in the same style, will make serious encroachments on the domain of *ignorabimus*. Beginning with the amœba, he explains how structure and consciousness gradually become un-

folded into the complex mind and body of man. Dr Clevenger's views are entirely materialistic; but though despising old superstitions, he appears to be somewhat credulous of new theories.

Dr Clevenger gives himself some credit for having furnished an original explanation of the sexual appetite. It is primarily derived from hunger. The cannibalistic amoeba probably impregnates itself by eating one of its own kind. "Crabs have been seen to confuse the two desires by actually eating portions of each other while copulating." The mantis and some of the female arachnidæ have been seen to devour the male. Traces of this taste in women have been recorded in the Latin language by a learned writer in *Natura*. The morbid appetite of pregnant women and hysterical girls to eat chalk and slate pencils is a longing transmitted from the times when their ancestors were oviparous reptiles.

Dr Clevenger complacently observes that "the hunger theory accounts for the female desire;" but upon what principle, he asks, can we have the corresponding male desire? We are sure our readers will be pleased with the following lucid explanation of this apparent difficulty:—"The adjustment of the relative male towards perpetuation involved his being finally eaten to enable him to survive at all; and, with the rapidity of his new birth and fusion with the female, it is conceivable that in this fusion an indifference to the process would ensue from the chemical aspects as to which ate the other, the only question being one of feasibility. The impediment to the process would be the ectosarc degree. The one which had less of this impediment would be the eater, the one with more the eaten."

But though some of Dr Clevenger's guesses are sometimes ludicrously insufficient to answer the riddles of nature, many of his observations are most ingenious. He shows an acute and penetrating mind, deeply learned in the structure and functions of the nervous system throughout the animal kingdom, and he has evidently long pondered over the problems of psychology. To one interested in such matters the book is a readable one. The form of expression, though sometimes uncouth, often has a vigorous terseness that sums up much in a short sentence. None can read the book through without gaining new ideas and having his thoughts set agoing on many subjects. Connected with the mysteries of mental growth and mental action, Dr Clevenger is profuse of acknowledgment to Darwin and Spencer; but many original ideas occur in his pages. His explanation of the reason why in man valves are found in some veins and not in others is striking. We can only give a part of it:—

"Place man," he says, "upon 'all-fours,' and the law governing the presence and absence of valves is at once apparent, applicable, so far as I have been able to ascertain, to all quadrupedal and quadrumanous animals: *Dorsad veins are valved; cephalad, ventrad, and caudad veins have no valves.* The apparent exceptions to this rule, I think, can be disposed of by considering the jugular

valves as obsolescing, rendered rudimentary in man by the erect head, which in the lemur stage depended. The rudimentary azygos valves may be a recent creation, and an explanation of their presence may be found in the mutability of the cardinal system. The azygos veins are derived from the intercostal, and the rudimentary valves may be a remnant of the original condition. The single Eustachian valve, being large in the foetus, has a phylogenetic value.

"The only reason I can assign for the absence of cephalic and cervical valves generally, while the jugulars possess them, is that the jugular system was the most important to our quadrupedal ancestors with dependent heads, hence valves developed in them; and owing to the cranial bloodvessels developing *pari passu* with the cranium and its contents generally, largely after man had assumed the erect position, the valvular formation elsewhere in the head would not occur while the jugular valves became rudimentary.

"Certainly valves in the hæmorrhoidal veins would be out of place in quadrupeds; but to their absence in man many a life has been, and will be, sacrificed, to say nothing of the discomfort and distress occasioned by the engorgement known as piles, which the presence of valves in these veins would obviate."

In a similar way Dr Clevenger shows that man, whose prototype he assumes once went on all-fours, has, by foolishly taking the erect position, exposed himself to inguinal and femoral hernia, from which our author tells us 20 per cent. of the human family suffer; but would he not have been more liable to umbilical hernia if he had continued to progress in the primitive way?

"If man has always been erect from creation," observes Dr Clevenger, "then we have nothing to hope from the future by way of an alteration of this defect;" but having a sure faith that our ancestors resembled *cetera animalia prona*, as Ovid has it, Dr Clevenger hopes that the so-called conservative influence of nature, which will tend to pile up additional muscular tissue in this region, by reason of the increased blood supply to that part, aided by natural and sexual selection, will eventually reduce the percentage of ruptures greatly, if it does not finally correct the trouble altogether. Towards the end of the chapter he relapses into pessimism. "At present," he writes, "the world goes on in its blindness, apparently satisfied that everything is all right because it exists at all, ignorant of the evil consequences of apparently beneficent peculiarities; vaunting man's erectness and its advantages, while ignoring the disadvantages. The observation that the lower the animal the more prolific would eventuate the belief that the higher the animal the more difficulties encompass his development and propagation; and the cranio-pelvic incompatibility alone may settle the Malthusian doctrine effectually for the higher races of men through their extinction."

Lectures on the Diagnosis of Diseases of the Brain. By W. R. GOWERS, M.D., F.R.C.P., Assistant Professor of Clinical Medicine in University College Hospital, and to the National Hospital for the Paralyzed and Epileptic. London: J. & A. Churchill: 1885.

It may not be easy, as one of the reviewers of a recent work sententiously remarked, to write a first-rate book on nervous diseases, but for the author of the work now before us it is impossible to produce anything except of the highest excellence. Each new work which bears his name is eagerly welcomed by his professional brethren, and the cause of this hearty appreciation is not difficult to find. In addition to the perfect acquaintance which Dr Gowers possesses with all the recent work which has been done on the nervous system, he brings to the consideration of nervous diseases a calm and judicial mind, so that he is enabled to weigh the facts and theories which have been or are being advanced, and assign to each its true clinical value. As an example of this, we may cite his remarks on the experimental researches on the functions of the brain by Hitzig, Munk, and Ferrier. "Here also, however, we meet with contradictions; and here also more has been ascertained than we can at present apply. Our guide in this department must be the facts of clinical and pathological observation. We must beware of applying wholesale to the human brain the conclusions derived from animals. The latter are of value to us only as indications for observation on man, and by enabling us to give a fuller interpretation to the facts we learn by our study of disease during life and after death." This scientific spirit pervades all Dr Gowers's writings, and, aided by his masterly arrangement and agreeable style, must be held to constitute a high claim to that confidence which is deservedly placed in him by the profession.

The present work in every respect will not only uphold but augment the author's reputation. It consists of lectures delivered at University College Hospital, which are now republished almost as they were delivered. There can be no doubt that the subject of these lectures is beset by problems of much greater complexity than any which had to be taken up in his previous work on the "Diseases of the Spinal Cord." These difficult questions, however, only serve to bring out Dr Gowers's clinical acumen and originality of views, but at the same time they necessarily make the book much harder reading than its predecessor.

The lectures deal in succession with the medical anatomy of the brain; the symptoms of brain disease, classed under the motor, the sensory, and the mental functions; affections of speech; headache, and other sensations; temperature, pulse, and general indications; ophthalmoscopic changes; the diagnosis of the seat

of the disease, and the diagnosis of the nature of the lesion. It is, of course, quite impossible to deal adequately in a short notice with the contents of the work, and we shall have to be content to touch upon some of its more salient features, although it is of necessity extremely difficult in a work of such uniform excellence to single out any one portion as more worthy of consideration than the rest.

The author does not put forward any claim to original research or observation in this work, but it is none the less true that it is characterized not only by the impress of the author's own individuality, but also by many a new fact and fresh view. Dealing, for example, with the pathway of taste sensation, he states his belief that it will be found that taste-impressions solely reach the brain by the roots of the fifth nerve, and "that the doctrine that the roots of the glosso-pharyngeal nerve have anything to do with taste is a curious physiological myth, due to too wide an induction from certain anatomical facts, and from dubious experiments on animals." His belief is based on the facts obtained from three cases of isolated paralysis of the fifth cranial nerve, in all of which there was complete unilateral loss of taste throughout the whole length of the tongue.

The explanation also of certain irregularities of hemiopia as being caused by variations in the decussation at the chiasma seems to us to be new, while the theory of abductor paralysis in lesions of the recurrent laryngeal nerve is entirely original.

In treating of the affections of speech the author is perhaps at his best. This complicated subject is put in such a clear light as to be easily grasped by the reader, the symptoms being grouped under the heads of sensory and motor word-processes. Dr Gowers, we may note, is evidently of opinion that our knowledge is as yet too fragmentary to allow of dogmatic theorizing, or to warrant rigid systematizing. At first sight we were somewhat struck by the absence of any reference to some recent Continental work on aphasia believed to be caused by lesions of the island of Reil (the aphasia of conduction of Wernicke). Close study of this part of the work, however, has shown us that Dr Gowers is of opinion that our first task is to realize the limits of our knowledge of aphasia, and that on such a subject as this, where the facts are so contradictory, we can at present only suspend our decision. The perusal of this portion of the work we must confess frankly to have given us much clearer views of aphasia than we had before.

We would very strongly recommend every practitioner to read the fifteenth lecture, in which the author deals with thrombosis of the cortical veins of the brain, and which contains a vivid sketch of this condition as it occurs in children especially. "These cases," he concludes, "are not at all infrequent. They constitute the majority of cases of sudden cerebral disease in childhood. The

hemiplegia may be transient, when the lesion is near, but not in, the motor part of the cortex; and probably, in early infancy, the initial hemiplegia, if slight in degree, may altogether escape observation. Most cases of epilepsy dating from infancy, in which the fits are one-sided, are of this character. We can very often trace slight indications of hemiplegia, in trifling weakness, slight shortening of the limbs, or in a slight degree of the mobile spasm that is so marked in the more severe cases."

When pointing out the mistake of assuming that when a diseased condition of the brain is due to syphilis it can be cured, the author reminds us, and the warning is useful to all, that such kinds of damage as necrotic softening caused by diseased vessels cannot be cured. He enunciates a law that speaking generally "the more quickly the changes are produced (and the symptoms developed), the less are they influenced by treatment; and in sudden lesions, as a rule, no influence is possible. Neglect of this consideration," he adds, "often brings discredit upon medical men. I have seen many cases of sudden hemiplegia, due to syphilitic disease of vessels, in which the patients were assured that if they were properly treated they would certainly get well; and when, after several years, there was almost no improvement, they were naturally inclined to think lightly, and sometimes to speak strongly, of the medical man who had given a prognosis so erroneous."

A pleasant little touch of satire is levelled at certain tendencies of medical practice, such as the use of soft and soothing euphemisms. "It has of late become fashionable," he remarks, "to call the partial loss (of power) 'paresis,' a term of doubtful value except as a means of giving a questionable satisfaction to patients who find comfort in the mysterious word, and think well of its donor—at any rate until the next physician whom they consult assures them that the disease is 'paralysis,' and that 'paresis' is only Greek for weakness."

Although this work is hard reading, its perusal has nevertheless been attended by a high degree of pleasure and a great amount of profit to ourselves. None of our modern writers has a greater power of bringing knowledge to a focus, so to speak, than Dr Gowers, and, as we have already remarked, we consider this present work to be a worthy sequel to its predecessor. That it may have as hearty a reception from the reading world is our sincere wish.

Exenteration of the Eye: a Substitute for Enucleation. By MID-
DLETON MICHEL, M.D. (Reprinted from the Transactions of
the State Medical Association of South Carolina, with an
Addendum, for 1885.)

In this paper the author gives in detail an account of three cases

in which he has performed the operation of exenteration—which consists in removing the cornea and turning out the contents of the globe, leaving the sclerotic with its muscle attachments and connective tissue capsule intact. No mention is made of Alfred Gräfe, who not only introduced this operation, at least in so far as its more general application is concerned, but whose experience of it is vastly greater than Dr Michel's. Notwithstanding the small number of cases operated on, the author considers himself entitled to put forward the following conclusions:—

"First, That sudden, entire, and early removal of all ocular contents, without involvement of the orbital structures, is followed by characteristic, harmless, serous chemosis, which soon disappears.

"Secondly, That excessive bleeding during exenteration occurs in recent traumatism, but is absent where marastic changes of the globe have already supervened.

"Thirdly, That effective severance of vascular and neural affiliation with the opposite eye at the *fons et origo mali* arrests sympathetic ophthalmia, even when this has commenced.

"Fourthly, That the simplicity and rapidity of a harmless exenteration, with its available stump for prosthesis, places it within the reach of any ordinary surgical skill.

"Fifthly, That if exenteration should fail under extraordinary circumstances to abort sympathia, the graver operation of enucleation would only hasten disastrous results.

"Sixthly, That exenteration should in future supersede enucleation, the irreparable resultant deformity from which latter operation is only justifiable in cases of malignant disease."

When Dr Michel has performed a larger number of exenterations, he will probably find, as we have done, that the greater or less chemosis, which invariably follows the operation, is sometimes associated with very considerable pain to the patient, and constitutes one of the drawbacks to the operation. Whilst there is no difficulty in arresting the "excessive" bleeding by pressure, there is a great tendency to oozing while the sutures are being applied, so that blood is retained in the sclerotic cup to an extent which not infrequently adds to the subsequent discomfort, and may even necessitate reopening. On this account we have found it advisable, not only as recommended by Gräfe, to confine the sutures to the conjunctiva, but also to insert a small catgut drain. As to the third conclusion, we hardly think that Dr Michel's experience is sufficient to justify him in putting forward this view, which Alfred Gräfe himself still considers *sub judice*. There is no evidence, from his description, that the case on which he relies as supporting his conclusion was one of actual sympathetic inflammation. Besides, the present state of our knowledge as to the mechanism of the transference of inflammation from one eye to the other points so strongly to the optic nerve being the channel

along which it takes place, that from this circumstance alone it seems questionable whether any operation which does not comply with the indications of, so far as possible, interfering with this channel is strictly justifiable in any case where the conditions known to be likely to give rise to sympathetic inflammation exist. But, apart from theoretical considerations, how many surgeons would submit to an operation on their own persons, the result of which, so far as the removal of sources of sympathy is concerned, is still uncertain, as a substitute for one which long experience has shown to be all but universally successful in this respect when performed in time? And this even were the advantages of exenteration from a cosmetic point of view greater than a little experience of the definite results shows them to be. We do not wish by any means to discountenance the operation of exenteration, which we believe to be a valuable addition to ophthalmic surgery, but merely to point out that there is a possible danger in adopting it, as Dr Michel proposes, in all cases except those of malignant disease. Undoubtedly the results, both with respect to the sinking in of the upper lid when no artificial eye is worn, and also to the movements of the pad on which the artificial eye rests, are generally superior to that of enucleation, although the shrinking, which gradually takes place, renders the eventual condition of the stump less satisfactory in these respects than its appearance shortly after operation seems to promise.

Atlas of the Cutaneous Nerve Supply of the Human Body. By JACOB HEIBERG, M.D., etc., Professor of Anatomy at the University of Christiania, Norway. Illustrated by ALFRED FOSTERUD. Translated and Edited, with Annotations, by W. W. WAGSTAFFE, B.A., F.R.C.S., etc. London: Baillière, Tindall, & Cox: 1885.

For clinical purposes, and as an aid to students in the dissecting-room, a clear series of plates illustrating cutaneous nerve distribution is likely to be of much service. Dr Heiberg has made a step in the right direction by introducing colour into his plates, and by publishing his atlas in a handy form. The work consists of ten beautifully finished Plates, with the various cutaneous nerve areas mapped out in separate colours. Plates I. and II. illustrate general nerve distribution on the trunk and limbs; Plates III. and IV., on the head and face; Plates V., VI., and VII. are devoted to the upper limb and hand, and the remaining three Plates to the lower limb and foot. A brief explanatory text by the author faces each plate, and the translator has added notes to most of the plates as well as a list of synonyms of the names of the nerves where this is needed.

While the execution of the plates and general plan of the work

are excellent, there are several objections which must be taken to the nomenclature of the nerves and to the mapping out of the nerve areas. We find, for instance, in the first two plates that the nerve supplies of the upper and lower limbs are classed as "lateral territories." Surely this would confuse students of British schools of anatomy, who are taught to distinguish anterior and posterior primary divisions of spinal nerves throughout, with only lateral branches of the former in the dorsal region. In the thigh a considerable area is allotted separately to the obturator nerve; this seems a very doubtful distribution. The supply of the fifth cranial nerve to the skin of the face is not very accurately given, and no account is taken of Hilton's observations on the nerve supply of the auricle. It would be of great advantage to supplement the dissecting-room subdivision of nerve areas in the skin with such clinical results obtained when nerves or nerve branches were known to be paralyzed.

On the whole, this atlas cannot be said to be as accurate and clear in its matter as it is artistic in design and praiseworthy in general plan.

The Anatomy of the Intestinal Canal and Peritoneum in Man. By FREDERICK TREVES, F.R.C.S., Hunterian Professor at the Royal College of Surgeons of England; Surgeon to and Lecturer on Anatomy at the London Hospital. London: H. K. Lewis: 1885.

THE description of the intestinal canal and peritoneum given in this work is based upon a systematic examination of one hundred fresh bodies.

The great advances that have taken place in recent years in the diagnosis of intestinal lesions, and the frequency with which operative treatment is resorted to, render it of great importance that we should possess an accurate knowledge of the complicated relations of the intestines and peritoneum. The accounts given in the various text-books have been repeated, with but little variation, through many literary generations.

The condition of the abdominal contents, as usually met with in the dissecting-room, is not favourable to very accurate observations, and Mr Treves has done well to avail himself of the vast resources of the post-mortem room of the London Hospital in examining the anatomy of this region. The results he has obtained form a valuable contribution to surgical anatomy. We can only refer to a few of these; but the whole work is well worthy of careful study.

Mr Treves found the total length of the intestinal canal in the adult to be very variable, and to bear no constant relation to age, sex, or height. In the new-born child it is pretty constant, but its rate of growth in childhood is very variable, a fact

which can readily be explained by variations in the diet and vigour of the digestive processes. In the new-born child the sigmoid flexure is relatively very large. It forms a loop 10 inches in length, while the rest of the large intestine is only a foot long. The fossa duodeno-jejunalis was found in forty-eight out of one hundred bodies. This fossa is of practical importance, as about fifty cases of hernia have been recorded in connexion with it. Mr Treves gives an elaborate account of the situation, mode of formation, and varieties of this pouch.

Not a single case of Meckel's diverticulum was met with either in the foetus or adult. The author agrees with Mr Birkett, that under normal conditions the coils of the small intestine cannot be drawn out of the abdomen below a horizontal line on a level with the spine of the pubes. It is not infrequent to find in women, at or past middle life, so loose and long a mesentery as to allow the bowels to be drawn from the abdomen some way below the line named. Mr Treves has carefully examined the holes in the mesentery within which loops of bowel are sometimes strangulated. He found the holes to be round, with thickened margins, and to be situated in the mesentery near the lower end of the ilium. In the foetus, and often in the adult, a portion of the mesentery between the ileo-colic artery and the terminal intestinal branch is thin, and destitute of bloodvessels or fat. He believes the mesenteric hole is usually formed by an atrophy of this part of the mesentery. The cæcum is usually described as being situated in the right iliac fossa, covered in front, below, and at the sides by peritoneum, but destitute of a serous covering on its posterior surface. The latter surface is connected by areolar tissue to the fascia covering the iliacus muscle. Sometimes, however, the posterior surface is covered by peritoneum, and the cæcum is connected to the iliac fossa by a meso-cæcum. Mr Treves's account differs in several important respects from the usual statements. He says that he never found the posterior surface of the cæcum uncovered by peritoneum, or connected to the iliac fascia by areolar tissue, and in not a single instance did he find a meso-cæcum. In a number of cases he found the cæcum entirely clear of psoas and iliacus muscles, and hanging over the pelvic brim, or even entirely lodged within the pelvic cavity. In every instance the cæcum was entirely covered by peritoneum, and free in the abdominal cavity. The cæcum is so frequently the seat of disease that it is needless to dilate upon the great importance of its relations to the peritoneum.

It is generally held that a meso-colon exists more frequently upon the right than the left side, and this is used as an argument in favour of left lumbar colotomy. In fifty-two out of one hundred bodies there was neither an ascending nor a descending meso-colon. In twenty-two bodies there was a descending meso-colon, in twelve an ascending meso-colon, and in fourteen a meso-colon

to both the ascending and descending colon. Mr Treves holds that both from developmental and comparative anatomy we might expect a greater frequency of descending than ascending mesocolons. The descending meso-colon is well marked in all species of monkeys, and even in the anthropoid apes. It is the remains of a primary fold of the peritoneum, while the ascending meso-colon is a secondary formation.

The Diseases of Sedentary and Advanced Life: a Work for Medical and Lay Readers. By J. MILNER FOTHERGILL, M.D.
London: Baillière, Tindall, & Cox: 1885.

THE author of this volume is well known as one of the most prolific workers in the field of book-making, but he has, unfortunately, never applied to himself the high aim with which Thucydides set out—not to write only for the present, but for future ages also. “To fill a gap in medical literature” is the mission of this book, but we fear this mission will not be fulfilled. Speaking frankly, we dissent from a great deal that is in it, and we think that its tone is calculated to lower the physician in the eyes of the public. As an example of the evil tendency which we deplore in the subject of the book, we may refer to the chapter devoted to the Seminary for Young Ladies. Not only is it prurient in thought,—it is ridiculously wrong in fact. It may have been written with a most laudable motive, but none the less might it have been suppressed with advantage. To give an instance of what we must call “bad form,” we need only mention the remarks in the chapter on Gerocomy (*sic*) with reference to the judgment passed on youth by old age. It savours more of the public billiard-room than of the private study.

Other singular tendencies of its author may be noted. “One day, casually meeting Dr B. W. Richardson, F.R.S., and falling into talk, etc.” Surely this is a sort of flunkeyism, closely allied to that of Bucket, the smart detective in *Bleak House*, who rejoiced in full titles, and always addressed his aristocratic patron as “Sir Leicester Dedlock, Baronet.” We are surprised after this to find that the author speaks of Christison, Simpson, Gull, Jenner, and Clark as if they were simple Knights Bachelor, while Sir Joseph Fayrer is ruthlessly stripped of his oriental order, and the great writer recently lost to us is familiarly spoken of as “Tom Carlyle.”

As an example of the English language, this book is in many respects a curiosity. Let us quote the opening sentence of one of the chapters:—“Another form of sedentary life which is apt to be suddenly developed is some accident limiting locomotion, etc.” There is something wrong here; and the unusual form of this and

other passages speak of hurried composition (if such writing at random deserves the name), and after-dinner work.

The book may perhaps be of some use to lay readers, and it will certainly amuse those who have any horsey proclivities; but for medical readers the Diseases of Sedentary and Advanced Life have yet to be written, and when they are we hope the result may be a contrast in tone to that of the present production.

History of Homœopathy: its Origin; its Conflicts. By WILHELM AMEKE, M.D. (of Berlin). Translated by ALFRED E. DRYSDALE, M.B. (of Cannes). Edited by R. E. DUDGEON, M.D. London: Published for the British Homœopathic Society by E. Gould & Son: 1885.

HAVING no desire to reopen a controversy which has long been closed, and shrinking at the same time from the expression of any statement capable of wounding tender susceptibilities, we have hesitated before entering upon a criticism of this volume. The fact, however, that to very many of our professional brethren the doctrines of Homœopathy are as the contents of a sealed book, is alone sufficient to induce us to venture on a few remarks. In doing so we must express our regret at the persistence among us, in this scientific age, of any dogmatic system, even when founded on quasi-rational grounds; but we are quite prepared to admit that such a result might have been averted if Hahnemann had been treated at once more generously and more philosophically by his fellow-countrymen. We entirely concur in the words of Hufeland, who is described even by Haizer as "ein Mann von den edelsten Eigenschaften des Characters," that "it is unworthy of science to treat the new doctrine with ridicule and contempt."

Many drugs, it is well known, have different or opposite effects in small and in large doses; but this is very far from being universal. It is, however, the only scientific basis for the law of similars; and this law can only be regarded as an example of too sweeping induction from limited experience. *Credo ut intelligam* was the dictum of Anselm and the watchword of Scholasticism; the law of similars is the creed of the Homœopathic system, and faith in it is the passport of its followers. Neither the one system nor the other has anything in common with the scientific spirit of modern times.

The present work is more interesting than most books emanating from its School on account of the large space devoted to the early days of Hahnemann, before he became the leader of the new sect. It is more defensive than aggressive in its tone, and may be said to be less one-sided in its statements than most books of its kind. It is impossible to avoid a smile at the term *allopath* as applied

indiscriminately to all who do not profess Homœopathy; and it is very funny to find the author objecting to the name of *homœopath* being given to men who profess the system. The physicians of this age are conspicuous for this above all things, that they reject all systems.

As might be expected, there is a certain amount of mud thrown at some of the great German teachers of the age; but perhaps not more than might be expected from the member of a persecuted sect. It is at all times a good thing for us "to see ourselves as others see us;" and if the reflection seen in the mirror held up in this book is not grotesquely distorted by some aberrations, it must to a certain extent "give us pause."

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH,

SESSION LXV.—MEETING II.

Wednesday, 18th November 1885.—Prof. GRAINGER STEWART, *President, in the Chair.*

I. ELECTION OF NEW MEMBER.

J. Murdoch Brown, M.B., C.M., was elected an ordinary member of the Society.

II. ADDRESS.

The President called upon Surgeon-Major G. H. J. Evatt, M.D., of the Army Medical Staff, to address the Society on "THE MEDICAL ARRANGEMENTS OF AN ENGLISH ARMY CORPS IN WAR, WITH SUGGESTIONS AS TO VOLUNTEER HELP."

Dr Evatt said he proposed to explain, in a very simple manner, the system by which Private Thomas Atkins when struck down by a bullet, say at Abu Klea, was cared for, transported to the rear, and thence to London or Edinburgh, as the case might be, and to show the various medical units and organizations through which he must pass. The medical system that now existed provided for this duty in a very complete way. It was not so in the past. The change that had been undergone was one that was of interest to civil and military men alike. He could not regard the question as a purely military one. It was essential that their professional brethren in civil life should be acquainted with the working details of army medical organization, and that they should especially know the weak points in it, if there were any. The reason for

this was that public opinion was wanted to carry any public reform, and public opinion on questions relating to the position of the army medical officer and his means of work could only be formed by their brethren in civil life. There were few civil doctors who were able to give them any help in their struggle for progress. Their views were not very clear about regimental bearers, the bearer companies, the field hospitals, and other important items of army organization. They could not appeal to the ordinary soldier, who had little knowledge of medical requirements, and therefore it was their duty, if possible, to educate their civil brethren. In the sheets that were distributed they would find a plan of the ambulance arrangements of an English army corps. When an English army took the field, everything was arranged on the army corps scale. The corps consisted of 36,000 men, and the quantity of artillery, cavalry, transport waggons, etc., were drawn up on that scale. The component parts of the corps consisted of three divisions, a cavalry brigade, and the "corps troops," chiefly artillery and engineers. In each division there were twelve military units—six battalions of infantry forming two brigades, one battalion of rifles, three batteries of artillery, and one company of sappers to prepare roads and do pontoon work. The corps was medically controlled by a surgeon-general, the division by a deputy-surgeon-general, and the cavalry brigade by a brigade-surgeon. Supposing, then, a bullet struck Private Thomas Atkins, of the first division, a thousand miles up the Nile, how did he come back to the Guards' Hospital in London. If they referred to the plan, they would see how this was done. When the army was mobilized, the battalion, which in time of peace consisted of about 400 men, was raised to a war footing, and to it was posted a surgeon ranking with a captain from the medical staff. This officer could, however, do very little in the field if he had not with him some trained assistants. Under the old system the bandsmen were used to do this in a rough way, but since the German awakening a number of men from the regiments were taken and specially trained to give first aid and to properly carry a wounded man off the field. These men were seen in most of the volunteer regiments of the present day. They belonged to the battalion, wore the regimental dress, and were known as the regimental bearers. They were not in any sense a medical unit. They formed the "regimental ambulance detachment," and went with the battalion surgeon, picking up the wounded, giving the first dressing, and carrying them off the ground. These men were allowed to every regiment of infantry; but so far as he knew (but he spoke under correction) there was no provision among the cavalry or companies of sappers, etc., for giving first aid to a wounded man. The number of men allowed for this first aid was two men from each company. They were equipped with stretchers and surgical haversacks. The surgeon himself saw the cases picked up, and when necessary had

them sent on to the rear by his regimental bearers. What was done with them here? In the volunteer army there was no organization to take charge of the wounded soldier carried off the field. The volunteers were in the same position, so far as the giving of proper attention to the wounded was concerned, as were the regulars at the Alma. Since then the English army had adopted the organization which had proved so valuable in the German army in its late wars. This was the Divisional Bearer Company. This company formed a unit in itself, and two were attached to each division and served under the general and principal medical officer of the division. One such company was attached to the cavalry brigade, and one to the corps troops. He claimed for these units that, if not perfect, they were at least the germs of the best unit ever yet devised by medical skill in the army. Each company was officered by surgeons who had full command, and were manned by men of the Army Medical Staff. For some time after their formation, so averse were the authorities to giving a command to the medical men, they were officered by captains and lieutenants of orderlies—men who were usually promoted from the ranks for efficiency. Sir William Muir, however, had brought about a much-needed reform by getting the command handed over to the medical officers. The lay officers of the companies now acted as quarter-masters. Each company was furnished with surgery and ambulance waggons, and water-carts. Each surgery waggon contained an operating tent and table, dressings, cooking utensils, and medical comforts. The bearer company then formed a highly mobile hospital moved up to within range of fire. It moved directly in the rear of the fighting line, and having pitched the operating tent and dressing station, and left a suitable staff to assist there, they sent forward the waggons to a "collecting station" further ahead. Here they received the wounded from the regimental bearers, and co-operated with them by sending on the stretcher-bearers of the company, who went on to the actual battlefield to give first aid and pick up the wounded that could not be attended to by the regimental bearers. At the collecting station the wounded were put into the ambulance waggons, and thence taken to the dressing station, where a more careful examination was made than could be done on the field, operations performed, and other necessary assistance given. The bearer company could not, however, stop here long. The army might have orders to march forward, and the company must march also to give assistance to other sick and wounded men. This made it necessary to have some place still further in the rear for the reception of those who had been attended to in the first instance at the dressing station. This was supplied by the field hospital, and the army in front was disencumbered of the wounded—a most important matter. Had such a provision existed for the care of these men at the Alma, our army might have pushed forward, entered Sebastopol with the

Russians, and there and then closed the war. As it was, forty-eight hours were lost because of the defective arrangements under which the wounded had to be embarked. There were four of these field hospitals to each division. Others called stationary hospitals were placed along the lines of communications. The line in the plan was represented as winding simply to save paper. He thought it necessary to mention this, as on one occasion, when explaining the plan to the men of the Medical Staff, and afterwards examining them, he was rather surprised to be told by one of them that the road was made a winding one in order that the wounded might have plenty of air on the march. These field hospitals were made to afford medical aid to 100 men. Each was in charge of four medical officers—one in command, one quarter-master who acted as store-keeper and also as paymaster under the medical officer. Under these officers were (by the arrangements of this year) two ward-masters, four storekeepers, two compounders, fourteen nursing orderlies, three cooks, one clerk, one messenger, two washermen, and five supernumeraries, five officers, and thirty-four men. The washermen were introduced into the personnel of an English field hospital for the first time this year. Previous to this no distinct responsibility for washing the clothes of the sick rested on the medical service. It was mixed up with the duties of the Commissariat, and much confusion existed at Ismailia on this head. The medical service was now responsible for this work in war time. There was no use talking of antiseptic surgery in war, unless they had the first and most requisite condition—a thorough cleanliness in the hospital. The five supernumeraries were necessary to act as watermen, as sewagemen, and so forth. It must be remembered that these field hospitals did not have the ordinary sanitary appliances that were found in all peace hospitals. The water was not brought in by pipes, nor the sewage carried away by drains; hence men were needed for this special work. Behind the field hospitals were the stationary hospitals on the line of communications. There were eight of these, each accommodating 200 men, allowed to the army corps. The field hospitals might be ordered to empty their sick and wounded, in order to march on behind the army and receive other sick and hurt. The watchword of medical organization in the army at present was, "Free the front of wounded men." These wounded were then brought by waggons or by trains to the stationary and base hospitals. On the Continent this was done by means of those ambulance trains which had been so skilfully devised by Baron Mundy for the Austro-Bohemian branch of the Maltese Knights. In England there were no such trains, so that waggons would have to be employed. They would have to use in each country the means of transport afforded them. Now, if they contrasted the regulations of 1882 for such an hospital with those of 1885 for a field hospital of 100 men, they would see that a great advance had been made. For such an hospital,

intended to cover 200 men, only 37 orderlies were allowed. For half that number 34 were now allowed. This was the result of the investigation made by Lord Morley's committee into the charges made against the medical department after the campaign in Egypt in 1882. The field and stationary hospitals of these past days were singularly defective in personnel; but as soon as public opinion was roused, the defect was supplied. There were no washermen, no sewagemen, and no messengers; but they had them all now. The result was shown in the success of the medical staff in the Soudan. The doctors had not changed in themselves, but the work was done more efficiently because they were better equipped. A distinct step forward in army organization had been made in that way. These field hospitals were not, however, tried in time of peace as they ought to be. One was pitched at Aldershot now and then for the training of the young surgeons, but there was no permanent unit of a field hospital kept for any English doctor to go and see in working order. It would be a great advantage to have such a permanent unit kept, with its horse, waggons, and other equipment. It could be tested every morning on parade, and its weak points discovered; and so it would be in an efficient state if called upon for war service. It was at least as important that the field hospital should be able to do its work properly as it was that a battery of horse artillery should be in constant readiness for service, and no one would think of leaving the battery untried till war was declared. Many army surgeons did not know anything of the equipment of a field hospital except from their books. A major of horse artillery would be considered rather incompetent who had nothing but book knowledge. They wanted to be put on the same footing as the rest of the army. Was not that a fair demand? They were not, however, likely to have their demands granted unless they were backed up by public opinion. This was an important matter, because unless their wounded were efficiently cared for, they were likely to come back from the war with a broken down morale; but the wounded men, who suffered most, would not come back at all. The number that did not come back from the Crimea was 18,000 men. The wounded passed along the line of communications till they reached the great receiver—the base hospital. A historic base hospital they had in Scutari. It had doctors of skill, but they could not give an order. There was plenty of science, but no soup. There was no medical staff corps, and of course there was chaos. The officer who would work such an hospital, consisting perhaps of 500 patients, was a perfect type of an army surgeon. He might get a telegram from the front at any moment, saying a battle was fought, and ordering him to be ready to receive 100 wounded men in two hours. He must do it. He must have 100 beds ready at the proper time for these men. If his beds were all filled, he must transport patients to the ship hospital to make room for those

coming in. He must have a very good overflow system and a highly efficient body of men at his command.

They now came to the last link in the chain—the hospital ship. If Baron Mundy had produced the best ambulance trains, the English people had produced the best hospital ships in the world. During the present year Brigade-Surgeon Gribbon had brought home in a P. & O. ship, the "Ganges," some hundreds of invalids most successfully. They had every medical comfort on the voyage. Since then a Crimean medical officer had told him that it was one of his duties to take home a number of wounded from the Crimea, and his whole medical paraphernalia consisted of one ball of opium. From the hospital ships the wounded were landed, and sent to one or other of the great military hospitals in England. The chain was now complete. It could not, however, be said that there was not room for improvement. He should like to ask the question whether in war time, with the battalion raised to its war footing, 1000 men, one surgeon was sufficient for its needs at the front? In foreign armies every battalion had two doctors in war time, and in the German army there was in addition a surgeon attached to the officer in command of every three battalions to act as his sanitary adviser. In Britain, however, they were weak in numbers, and it might be that two men could not be spared at this point. In reference to the strength of the bearer company it was an interesting question—Whether one surgeon was sufficient with a party of eight stretcher-bearers? What time would it take a surgeon to attend to eight wounded men lying on stretchers? There ought to be another doctor with the stretcher party. Then, was not the collecting station to which all the wounded men were brought too important to be under the command of only two sergeants? It seemed impossible to allow so important a place as the "collecting station," where wounded were certain to accumulate, to be cared for by any sergeants, however careful. The presence of a medical officer seemed to him absolutely essential, and at least four private orderlies in addition to the sergeants to work at loading and placing the wounded in the ambulance waggons, or for placing the wounded on the mule cacolets and litters—a very trying and tedious work. They thus seemed to need, even in the front of the dressing station, an increase of four orderlies and two doctors. At the dressing station itself an extra surgeon was needed to assist at the operations, and an increase of private orderlies for cooking, caring for the accumulated wounded, and for water carrying—say at least ten men were needed to make the dressing station efficient. All this would add to each bearer company three doctors, and some fourteen or fifteen orderlies. Without a certain further increase, the present bearer company could not work well in war time. The Medical Staff Corps was, however, insufficient in itself to do all this work for a complete army corps in the field. A proposal had been made that

to overcome this difficulty there might be formed a Militia Medical Staff Corps, which could be called out each year for training in ambulance and nursing duties. They could be formed into district militia companies of the Medical Staff Corps, and dressed in medical uniform, and regularly trained every summer at the district headquarters in all military ambulance and nursing work. Two thousand such men trained yearly would be of enormous help in any foreign war. It was essential to put these men into medical corps uniform, so that their *esprit de corps* might be increased, and that they might take increased interest in their work. For Scotland, probably, 200 such men would be under training, and they could assemble every summer at Edinburgh, or other military centre, and go through all ambulance and field hospital training.

He attached much importance to an efficient militia medical organization as economical, and as being ready at once when war occurred to take the place of the regular medical corps. He came now to ask how the volunteer army stood in relation to this question? They stood exactly where the regular army did on the 20th September 1854. They could not be called an army. They would be altogether unable to deal with their wounded as armies of the present day were. They had only the regimental surgeon and bearers. There was no bearer company. It was essential that there should be a trained bearer company in each county for the volunteer army. These trained men might be useful beyond the sphere in which they were trained. They might be counted on to supply places in the regular army in war time. To work this volunteer service properly they wanted only officers. These they must have from the medical profession, and on it lay the whole responsibility. Already a step had been made. They had begun to train medical students, and the training would be most beneficial to them. In the civil hospitals they were not taught hospital organization as they would be in a medical staff corps. The discipline would also be of great benefit to them. When trained they would furnish the country with a body of young red-cross doctors. It might be thought a waste of time and quite unnecessary to teach a medical student how to carry a stretcher, but the student must learn all these things. He must learn to obey in order that he might command; for the army doctor of to-day must be not only physician and surgeon, but must be able to make the machine go. The students would also be the future officers of these bearer companies. With the most intense self-sacrifice this work had been begun among the medical students of London by Mr Cantlie. He had already trained 360 of them, and 100 went to Aldershot in July to receive training along with the privates of the Medical Staff Corps. This volunteer movement had been aided in London by the formation of a Volunteer Medical Association. The volunteer officer could not go to the War Office

by himself, but when he formed an association with a number of people interested in ambulance work, he could make what suggestions he pleased, with the knowledge that they were more likely to be listened to and adopted. There should be in every English and Scotch county at least 10 officers and 100 men of a county division of the Volunteer Medical Corps, in addition to all existing volunteer regimental surgeons. These county companies would be dressed in medical uniform, be officered by non-regimental volunteer medical officers, and should form an administrative battalion for the division. To this battalion a brigade-surgeon of volunteers would be posted as commandant, and an officer of the Army Medical Staff should be attached as instructor and adjutant. The whole would be under the general command of the Principal Medical Officer of the district. This would equip the volunteer army with the elements of medical aid; and no doubt whatever, in war time, officers and men in certain numbers might come as volunteers for war. What was wanted, above all things, was disciplined trained war aid, and by carrying the ambulance work into the schools this could be achieved. A military volunteer medical reserve would thus be formed, recruited from young surgeons of the house-surgeon class, who, with a volunteer commission and good pay and allowances and ample reward, would come to the Army Medical Staff for war. This war reserve would be enrolled yearly by the War Minister sending out circulars every December to the schools, and inviting young medical men to promise to come to war, if any occurred in the ensuing year. In the following December the surgeon would be again free. There was ample room in our army corps scale for volunteer doctors, if trained and disciplined. One such officer might be posted to each battalion as a second doctor. One or two might be attached to the bearer company of each brigade. One might be posted to each stationary hospital, and there was room for several in the transport work on the communications. Underlying all lay the questions of discipline, *esprit de corps*, and morale. If these existed all might go well, without them all was impossible.

The medical service was to-day evolving itself out of former chaos. That evolution would be aided by the sympathy of the civil profession; that sympathy must be based on knowledge, and to disseminate that knowledge he had given this very elementary lecture to-night. The civil profession were entirely ignorant of the military medical system; they knew little of the need of forethought and anxious pre-arrangement of all things needed for war. Hence the army medical service looked in vain for active intelligent help in its development. In the future it would not be so; the students to-day knew more of their work. They were sowing the seeds of future knowledge in their minds, and good fruit might one day come. They stood in the army true alike to the soldier and the profession of medicine; standing fast

by science, but also by discipline ; proud of medicine, but soldier-surgeons also. In that army they were not outsiders, but a corps, and an important corps, sharing every danger, and claiming just reward from England. With just and evenhanded treatment every success was possible, but without it any system, however good, must fail.

The President said he was sure he expressed the feelings of every member of the Society when he said they were much indebted to Surgeon-Major Evatt for the very able and interesting statement he had brought before them. For himself, he had to say that he was in a state of very dense ignorance on these matters. He felt that he had got a certain amount of knowledge in a wonderfully pleasant way, and in a very short time. It was delightful to see a man so full of enthusiasm tempered by good sense, and able to marshal his facts as Dr Evatt had done. Mr Cantlie was with them, and he hoped he might venture to call upon him to give his experiences in dealing with the London students.

Mr Cantlie said he should like to say a word or two as to the formation of a Volunteer medical association in Edinburgh. There was one formed in London, and it was to this or similar associations in the country that he looked for the bringing forward and developing, for it scarcely now existed, the Volunteer medical service. Destitute of an organized medical service, the Volunteer army must ever remain as a show or marching-past army. It was not to the credit of the medical profession that the splendid force now bearing arms in the Volunteer service should be branded as useless, because the doctors have not bestirred themselves. It was to this end that the Volunteer Medical Association was founded, and it was to create common action amongst the Volunteer surgeons that it was intended to found local branches of the Association throughout the kingdom. There seemed, however, to be an apathy among Volunteer surgeons about organizing themselves. He had recently addressed 3300 circulars to Volunteer surgeons throughout the country, and from these he had only 47 responses. The parent Association in London had tried to bring the Volunteer surgeons forward, but had almost given it up in despair. The scheme in the last circular was that there should be a number of associations formed throughout the country, looking upon the Association in London as the parent. In this way they would be able to bring the claims of the surgeons before the authorities. Many things had to be asked for: such as proper rank, promotion, the formation of a staff, employment of and pay when during war the civil doctors were called in, the question of equipment, formation of an army medical reserve, and such like, that could be carried through only by the combined voice of the Volunteer surgeons generally. The medical profession in this country was split up into two sections, the civil and military.

The members of either party know little of, and care less about, the working of the other. The special knowledge and training of an army surgeon were lost to the civil profession. There were 800 men of the Army Medical Staff who were specialists in hospital organization and sanitation. How many practitioners in civil life knew anything of such things? Hospitals were left to secretaries and governors, and if anything went wrong with their patients' houses they must have a "man" to look to the drains, etc. The civil surgeon knew when the drains of a house or hospital were out of order when his patients died of pyæmia or diphtheria, and he knew that they were all right after they had been seen to only by his patients not dying of such diseases when the ward was reopened. The ability to test the drainage, or to know anything about it, he did not consider his business, and he had had no training therein. Then, again, in regard to hospital administration and organization, What did the surgeons and physicians who lent their services to these institutions know of such matters? It was left to laymen, and the doctor only knew something was wrong financially when a ward was closed or some item in drugs or comforts had been forbidden. Now, the only branch of the medical profession which had received any training in such matters, the army surgeons, they, the civil practitioners, had completely ostracized. In this country, it might be fortunately, the civil doctor did not have an opportunity of distinguishing himself in war. On the Continent it was different. Who would have heard of Esmarch of Kiel, a surgeon in a fourth-rate German town, had it not been for his war services? Langenbeck, Reyher, and others had been brought to the front by their war services. It was not, however, the glory of attending the wounded on the field of battle which continued these men in the position they were. No; it was the training in organization and hospital administration which gave them a power in their respective countries, such as no untrained civil practitioner could ever hope for. But, he said, they threw away 800 valuable men, and let them wear their lives out on pensions, instead of either imitating them in their training, or bringing them amongst them and making them useful to them. What caused so many hospital scandals where the doctors had to do with management? Not want of scientific skill, but absence of the bare elements of system and administrative powers, which could not come without training. It was to be hoped that one day such form of teaching and training would be inculcated upon every medical student; and that by-and-by the medical profession in this country would be taught to walk by itself and manage its own affairs. Many young surgeons had tendered their services at the head-quarters of the Volunteer Medical Staff Corps in London, but they could not see the advantage of going through any form of drill or previous training. Their profession, said they, was similar in the civil or military hospital, on the field of battle or

elsewhere. That was an example of the opinion of the most intelligent of young civil practitioners, and it was on a footing with that held by the medical profession generally in Britain. In every country in Europe except our own every doctor had to join the army, and he thereby received a training in system and administration which served him in good stead for the rest of his life. In regard to the formation of a Volunteer Medical Staff Corps, that was a thing of secondary importance to the development and organization of the Volunteer medical service generally; but they could not have officers fit to undertake any such work unless they were previously trained, hence it was that the medical students, as future officers, were first called upon to undergo a training. In London very nobly had they responded; and at the present moment there were 300 medical students and about 100 laymen, formed into four companies, in a high state of efficiency. The Government had granted an adjutant and four instructors, besides the usual money grant, for the purpose of training the battalion. It was to be hoped that two companies—one of medical students, the other of laymen—might be formed in Edinburgh, a third in Glasgow, and a fourth in Aberdeen. As soon as these were recognised, they would have an adjutant and instructors granted for Scotland. The two companies outside Edinburgh would be recognised as outlying companies of the corps, the headquarters of which would be in Edinburgh. Pressure could only be brought to bear upon the authorities by a body of influential men in a district banding themselves together and representing their wants to the proper authorities. Individuals could do nothing with State authorities; but were a branch of the Volunteer Medical Association formed, consisting of influential laymen and doctors in Edinburgh, the Volunteer surgeons could command attention to their wants in terms which must command a hearing. He would suggest that as soon as possible a branch of the Volunteer Medical Association be formed in Edinburgh.

Dr Wolseley, P.M.O. for the North British District, said Drs Evatt and Cantlie had gone over a wide field. In the days when he first joined the service, the junior medical officer or assistant-surgeon went into the field four paces behind the colours. He was four paces behind them at the Alma, and he did not think he was worth twopence there. They had now a very different system. They had worked out from this state of disorganization a system that was nearly perfect. He did not altogether agree with some of Surgeon-Major Evatt's suggestions. He thought, in particular, that two surgeons to a battalion in war time were too many. The surgeons were too valuable to be put there. The formation of a Volunteer Medical Association was a practical point, and he should do what he could to help the movement. He had that day seen the General commanding the district, and

told him of this meeting and its object. The General authorized him to say that it met with his approval, and would have his support in every way. He trusted that the matter would not end in words.

Dr P. A. Young said it cheered the hearts of those who had been working long at this ambulance organization to have such addresses as they had listened to that evening from Surgeon-Major Evatt and Mr Cantlie. He had acted with the German army in the Franco-Prussian war, and had been so struck with their organization that he had, on returning to this country, written to the public papers urging its adoption in our own army, regular and volunteer. He had particularly advocated that the volunteer surgeon, instead of simply marching in the rear, should train members of his corps in ambulance work. He held that, instead of training only two men from each company, every man should receive the rudiments of such training. Every rank and file in the German and Austrian armies was taught the use of Esmarch's bandage. If a soldier were wounded in a large artery he might bleed to death before the bearers reached him; but if his comrade knew how to apply the tourniquet, his life might be saved. The volunteer army was still very far from perfect in its medical department. They had done a little to remedy that defect, having formed four bearer companies in London, and last year they had attempted the formation of such a company in Edinburgh. They hoped to have another company formed of laymen next spring. Such a company could be readily formed by the men trained under the St Andrews Ambulance Association. The volunteer surgeons could help forward the movement by forming an association. They could speak with more confidence collectively than individually.

Mr Cathcart said the students in Edinburgh had not been behindhand in endeavouring to organize a Volunteer Medical Staff Corps. They had had, however, to fight under the greatest possible difficulties. They had no funds nor other appliances, no stretchers. They had to find their own instructors, and those of them who acted as officers had to pick up their training as best they might. The students had felt this disadvantage, and their original numbers had lately dwindled down considerably. They had still forty or fifty members on the roll, and that afternoon there had been another enrolment. If, however, they were to continue and be efficient they must have some backing up. The general public must help them with funds and interest. The formation of a Volunteer Medical Association would be a great help.

Dr Caverhill gave a short account of the ambulance training in the different yeomanry regiments in Scotland. He had introduced into the East Lothian and Berwickshire Yeomanry Cavalry what was called the Victoria Cross competition; this was

intended to train the troopers to rescue their wounded comrades lying in positions of great danger. It consisted in:—1. Riding at full gallop over a hurdle 3½ feet high; 2. Decapitating a dummy enemy; 3. Dismounting and placing a figure of a wounded trooper on his horse; 4. Remounting; and 5. Galloping back with the figure astride in front, and jumping the hurdle on the way. The competition was very popular, sixteen of the troopers having become most proficient in it. He urged that they should press on Government the necessity of establishing large camps annually or periodically, where all the different arms could be combined, and an opportunity afforded the auxiliary forces of learning practically something more than mere parade movements; the camp to be supplied with a field hospital where the bearer companies could be efficiently trained in its different departments. He should like to ask Surgeon-Major Evatt if there was any organization for giving first aid to the wounded cavalry in the regular army?

Surgeon-Major Evatt thought Dr Caverhill had raised a very important question. He did not know what provision, if any, existed for the giving of first aid to a wounded cavalry soldier. The only experience he had on the point was that when he went out to Suakim in February, having landed just before MacNeill's zereba fight, an order came to him to join the cavalry brigade, and march to Hasheen. He at once asked for an escort, and obtained twenty-five hussars. But the bearer company he commanded was entirely on foot, and equipped as infantry, and could not possibly keep up with quick moving cavalry. By the merest chance—as he was marching across the plain, quite detached from the cavalry brigade, with whom his dismounted men could not keep up—he met Sir Gerald Graham, who, seeing the dangerous position the company was in, ordered them at once into the square, where all the non-cavalry troops were safe from attack. Sir Herbert Stewart had written a highly interesting minute on the subject of the bearer company with cavalry; and no doubt whatever existed that the present infantry bearer company with cavalry was unfit for war work, and would certainly fail in any real warfare. What was needed was a well-mounted lightly equipped bearer company, with many mounted men, light ambulances, and horses with cacolets and litters, and very light equipment. This company should bear the same relation to the ordinary bearer companies that a battery of horse artillery bears to a field battery. The question also existed if one surgeon per cavalry regiment was sufficient in the field. The duty was heavy and exhausting, and two men at least needed. In all these questions enlightened public opinion was needed, and it was only by such enlightenment that real war efficiency could come. In conclusion, he desired to thank the Society for the very kind reception he had received.

The President moved a very special vote of thanks to Surgeon-Major Evatt for his address. This was unanimously agreed to.

Colonel Jones, Q.E.R.V.B., moved a vote of thanks to the President and Council of the Society for the kind invitation they had given to the volunteer officers to hear the interesting and able address by Surgeon-Major Evatt.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XLVII.—MEETING I.

Wednesday, 11th November 1885.—Dr J. CONNELL, *President, in the Chair.*

I. *Dr Foulis* exhibited a new form of CATHETER for washing out the bladder.

II. *Dr Leith Napier* showed (1) a CANCEROUS OVARY, removed by laparotomy sometime previously; (2) a PLACENTA, with velamentous insertion of the cord.

III. *Professor Simpson* showed the UTERUS and APPENDAGES OF A CORD. The specimen showed distinct evidence of ovaritis, giving rise to inflammatory adhesions to the surrounding organs.

IV. *Dr Alexander Sinclair* showed the OVARIES and TUBES from a Tait's operation which he had recently performed. The patient was the subject of hystero-epilepsy and of profuse menorrhagia, both of which had disappeared since the operation.

V. The retiring President, Dr Connel, gave his VALEDICTORY ADDRESS (which appeared at page 577 of this Journal). On the motion of Professor Simpson, seconded by Dr Leith Napier, a cordial vote of thanks was awarded to Dr Connel for his address.

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING I.

Thursday, November 12, 1885.—Dr LOCKIE *in the Chair.*

The officers for the ensuing session were elected as follows:—*President*—Stewart Lockie, M.D. *Vice-President*—Roderick Maclaren, M.D. *Hon. Secy.*—William Douie, M.B.

Dr Lediard showed a patient, previously exhibited, suffering from MENINGOCELE. The cyst had been tapped twelve times, and injected eleven times with Morton's fluid. Neither good nor harm had ensued.

Dr Barnes read a paper on RHEUMATIC HYPERPYREXIA.

Dr Lockie said that the older authors had included under the term cerebral rheumatism cases of rheumatic meningitis, and also cases of rheumatism affecting the nervous system, where paraplegia or hemiplegia were replaced by articular rheumatism. In rheumatic hyperpyrexia the heat-controlling centre was probably affected by the rheumatic poison. Cold in some form was the only successful treatment. Some cases were so rapid as to be moribund before the treatment could be carried out.

Dr Lediard had witnessed some cases of acute rheumatism in which hot packing was used. The results were most pernicious. One case died of hyperpyrexia after hot packing.

Dr Murphy alluded to the pain and griping which followed the injection of large quantities of iced water into the rectum.

Dr Sullivan alluded to the rarity of hyperpyrexia in acute rheumatism.

Dr Barnes said that in the Middlesex Hospital 20 cases of hyperpyrexia had occurred in 1300 cases of acute rheumatism.

Dr Maclaren showed a large PAROVARIAN CYST removed from a woman aged 30. She had been tapped six years ago, when thirty-one pints were drawn off. In two years the fluid had begun to reaccumulate. Her size was enormous. The operation was very simple. Two children had been born since the tapping; the last two years ago.

Dr Greenlees showed a DIVERTICULUM OF THE SMALL INTESTINES discovered post-mortem. There were no symptoms.

Dr Lediard showed a MELANOTIC SARCOMA of the finger-end, growing from the periosteum.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

STROPHANTHIN.—In the *British Medical Journal* for 14th Nov. there is a very able paper by Professor T. R. Fraser on the action and uses of digitalis and its substitutes, with special reference to strophanthus (*Hispidus*?). This plant is a native of equatorial Africa, and belongs to the Natural Order Apocynaceæ. It contains a crystalline principle which Professor Fraser has called strophanthin. It is a non-nitrogenous body, and exists in the leaves, bark, and seeds of the plant, but is most abundant in the seeds. After

describing the chief physiological actions of the drug, Professor Fraser says:—"It was found that solutions of digitalin, of 1 part in 100,000, of 1 part in 50,000, of 1 part in 10,000, of 1 part in 5000, and of 1 part in 4000, each produced characteristic changes in the heart's action, but were not sufficiently strong to kill the heart—at any rate, not within two hours. With strophanthin, on the other hand, a solution of 1 part in 100,000 quickly stopped the heart's action in extreme systole, characteristic changes in the heart's action having previously been produced. I then increased the dilution, and solutions of 1 part in 250,000, of 1 part in 500,000, of 1 part in 1,000,000, of 1 part in 2,000,000, of 1 part in 3,000,000, of 1 part in 4,600,000, of 1 part in 6,000,000, and of 1 part in 10,000,000, were each tried, with the result that the heart was characteristically affected, and killed by each of them. Even the almost inconceivably minute dose which was brought into contact with the heart when a solution of 1 part in 6,000,000 of strophanthin was used, produced complete stoppage of the heart's contractions in extreme systole in about twenty minutes. Assuming that the digitalin was a fairly representative specimen of that substance, these experiments indicate a very striking difference between strophanthin and digitalin in reference to their power of producing the changes in the heart's action, which constitute a most important feature in the therapeutic applications of the members of the digitalis group. I have, however, already stated that increase of blood-tension following the administration of these substances may be produced also by an action of the bloodvessels. We know that digitalis has such an action, and we know also that it is produced in part, if not entirely, by an action on the bloodvessels themselves. Employing the same digitalin as was used in the heart experiments above referred to, it was found that in a frog, whose central nervous system had been destroyed, a solution of 1 part in 20,000, passed through the bloodvessels, produced, in six or seven minutes, such extreme contraction of the vessels as practically to prevent the solution from passing any longer. Substituting in other experiments strophanthin for digitalin, a solution of 1 part in 20,000 had no appreciable effect, nor indeed could any decided effect be detected even when the strength of strophanthin solution was increased to 1 part in 3000; but a temporary effect, which was soon recovered from, was produced when the solution was increased to 1 part in 2000. Strophanthin, therefore, exerts a much more powerful action upon the heart, and a less powerful action on the bloodvessels, than digitalis. What the exact significance to therapeutics of this difference may be I am not prepared decidedly to say. On first consideration, however, the advantage seems to belong to strophanthin in the treatment of cardiac disease at any rate. In difficulties and embarrassments of the circulation depending upon a central cause, it seems preferable to act upon

the heart alone, rather than also to increase its difficulties by closing the bloodvessels into which it must empty itself. It may possibly be the case that some of the benefit which digitalis might otherwise cause is antagonized, as it were, by the action it so strongly produces upon the bloodvessels, and that this latter action may even render large doses dangerous when they are given to a much weakened heart. Now, gentlemen, allow me a word or two further as to the therapeutics of my subject. I regard, in common with many others, the characterizing action of the members of the group to be the action on the heart. They may be used with advantage in all weak states of the organ, whatever the cause of weakness; and their most satisfactory results are, I think, produced in mitral disease. They do little, if any, good when the cardiac muscle has undergone much degeneration. They sometimes fail even while degeneration is absent, when lesions of the orifices throw too great a burden upon the heart; and they are injurious when sufficient compensatory changes have been produced. They are valuable as diuretics; and it appears from the experiments I have described with strophanthus, that the diuretic action can be produced by substances which have only an insignificant action upon bloodvessels. They are antipyretics, but in regard to this action I have no special experience to describe. In the case of strophanthus I have occasionally seen pyrexia disappear; and very frequently the charts of my apyretic cases exhibit a subnormal temperature. The hæmostatic action which seems clearly to be possessed by digitalis finds a satisfactory explanation in the marked effects produced by it upon the bloodvessels. Experiments with strophanthus, however, show that it is not an action possessed by all the members of the group. And, lastly, as all the members of the group act directly upon the striped muscle, I see no reason to doubt that the ecboic action possessed by digitalis is an action common to them all. As to the inconveniences and disadvantages which are associated with their application in disease, an experience no doubt somewhat limited would lead me to state that sickness and gastro-intestinal disturbance are not produced so frequently by strophanthus as by digitalis, while I have not yet obtained any distinct evidence of accumulation, with its consequent manifestations of intolerance, in the cases in which strophanthus has been administered even for several weeks uninterruptedly. The therapeutical applications of digitalis are among the most valuable of those producible by substances used as remedies. The fundamental basis of these applications is to be found in an action possessed also by a considerable number of other substances. There is reason to hope that one or other of these substances may in some respects even be more valuable than digitalis as a means for applying to the treatment of disease the characterizing pharmacological action of the group."

PURE TEREbene IN THE TREATMENT OF WINTER COUGH.—During the last five years I have employed a method of treatment which yields excellent results. I have before me notes of 114 cases of winter cough, some taken at the Chest Hospital, others at Westminster, and others again in private practice. They were all treated with pure terebene, a substance prepared by the action of sulphuric acid on oil of turpentine. It is an agreeable remedy, being a clear, colourless liquid, with an odour like that of fresh sawn pine wood. It will not mix with water; but as the dose is small, it can readily be given on sugar. It is not the same as the patent medicine sold under the name "Terebene." The best method of indicating its sphere of action and illustrating its utility will be to give a brief abstract of one of the notes of one of the cases. This, it should be said, is a fair average case, taken quite at random:—R. N., aged 43, a commercial traveller, stated that he had been subject to cough every winter for twelve years. His work was against him, and he was a good deal exposed to wet and cold and the inclemency of the weather. His cough used to trouble him badly only in the winter; but year by year it seemed to be coming on earlier, and now he was hardly ever free from it. It came on in fits, which shook him to pieces; and it was always very bad the first thing in the morning, often making him retch and vomit. There was a great deal of phlegm, thick and yellow, when he was in the country, but speckled all over with black in London. It was difficult to get up, unless he could get some hot tea or something to loosen it. The shortness of breath was worse than all; for it prevented him from going about, and interfered with his business. He had never spat any blood worth speaking of, but there were at times streaks after a severe bout of coughing. He became no thinner, generally losing in the winter and picking up again in the summer. He had had a great deal of treatment; and mixtures, lozenges, and liniments without end. On examining the chest, it was found to be emphysematous, and there was a loud bubbling rhonchus at the base of each lung. On 1st November he was ordered ten drops of pure terebene on a piece of sugar every four hours. In three days he returned, and said there had been a marked improvement. The cough was easier, the phlegm was lighter in colour and not so thick, and the breathing was decidedly better. The dose was increased to 20 minims every four hours; and a week later the patient wrote to say that he was better than he had been for years, and was almost able to do without the medicine. I saw nothing of him again until 6th January, when, being in town, he came to see me. There had been some return of the old symptoms, and he was anxious for further treatment. I ordered him a small Maw's spray-diffuser, holding about an ounce, and instructed him to use it with the terebene as an inhalation several times a day. A fortnight later he wrote saying that he bought a larger apparatus, and that his com-

plaint was more amenable to treatment than it had ever been before. The terebene-spray eased the cough, brought up the phlegm, and, above all, relieved the shortness of breath. On his long railway journeys, when he was unable to use the spray without inconveniencing his fellow-passengers, he rubbed the terebene on his moustache and beard, so that it might slowly diffuse, and, as he said, softened the atmosphere. One of the great advantages of pure terebene is, that it is not a bulky medicine. An ounce bottle carried in the pocket will last for days, and is always ready for use. It is best to begin with five or six drops on sugar every four hours, and gradually to increase to 20 minims. This is for most people the maximum quantity, but the drug has little or no toxic action; and one patient was so enraptured with his remedy, that he insisted on taking a teaspoonful every four hours for a week. The only disadvantage I have ever noticed from its employment is that it gives a peculiar and characteristic odour to the urine, a circumstance which patients never fail to mention. When used as a spray, from 1 to 2 ounces should be diffused and inhaled every week. In some instances I have tried giving it mixed with an equal quantity of olive oil flavoured with oil of peppermint. In twenty-five cases I gave the terebene in the form of an emulsion, made, if I remember rightly, by mixing it with a little tragacanth powder, adding water, and shaking well. Each ounce of the emulsion contained a drachm of the terebene, and it was usually given in half ounce doses four times a day. The results were excellent, but not better than with the simple terebene itself; and I saw no reason for continuing the use of a more expensive preparation. In every case of winter cough in which the terebene-spray was used systematically, there was a marked improvement. In many instances it was noticed almost immediately; but in other cases, especially in the very chronic ones, the patient had to continue using his remedy for several weeks. Even when there was marked emphysema, with little movement of the chest walls, some benefit was experienced. I treated eighteen cases of phthisis by the same method, and the results were certainly most encouraging. It did most good when there was old consolidation, when no active mischief was in progress, and especially when there was no elevation of temperature. I have also used it as a dry antiseptic inhalation on the cotton-wool of a respirator in phthisis, and have been much pleased with the results. In one case, that of a young lady, the respirator was worn almost continuously night and day for nine months; and the right lung, which was almost breaking down, cleared up, the temperature becoming normal, and the cough and other symptoms subsiding. I have no doubt that pure terebene would be useful in checking hæmorrhage from the lungs, but on that point I have no experience. Many sufferers from winter cough also complain of acidity and flatulence. I soon found that the internal administra-

tion of pure terebene was an excellent remedy for this combination of symptoms. It checks the formation of flatus so quickly, and is so efficacious in expelling any that may remain in the stomach or intestines, that I constantly employ it in cases of dyspepsia when flatulence is a prominent symptom. Patients like it, and often continue taking it for months or years. It acts as an antiseptic much in the same way as glycerine, oil of cajeput, and oil of eucalyptus. I am surprised that it has not come more largely into use in the treatment of flatulence. Pure terebene is of such value in winter cough, that I rarely experience the necessity of resorting to other remedies.—Dr William Murrell, in the *British Medical Journal* for 12th December.

TERPINE.—The *Therapeutic Gazette* for November gives the following abstract, from the *Revue de Médecine*, of the researches of Lépine on Terpene:—Terpene, a crystalline hydrate, obtained from the oil of turpentine, is readily soluble in alcohol (in the proportion of 1 to 7). In cold water it is soluble in the proportion of 1 to 200; in water which has first been boiled and then cooled down to a lukewarm state, it dissolves with still greater ease. Lépine maintains that terpene has an action very similar to that of turpentine, but that it acts not only in smaller doses, but also is much better borne by the stomach than the latter. The crystals of terpene have neither taste nor odour, and create therefore no repugnance on the part of the patient, as is so often the case with turpentine. But these are not all the advantages which Lépine claims for terpene. Fluidity and ease of expectoration, our author declares, are the rule after terpene, but not after turpentine; and as this sort of expectoration is produced by terpene even if given in a small dose, this drug must be regarded as the best expectorant in existence. In a dose of 3 to 9 grains, the drug exerts the above-mentioned therapeutic effect with great promptness. Of 200 patients in whom terpene was exhibited, not one complained of any inconvenience experienced by the drug, while the majority felt so convinced of its utility, that they asked for its continuance. In two chronic cases even, which had been very rebellious and resisted every other medication, terpene, given in 2 gramme (30 grains) doses, brought prompt relief. It is a peculiarity of the drug that, given in a large dose, its action is just reverse from its effects produced when given in small doses (about 3 to 15 grains); i.e., it suppresses expectoration when given in large doses, and is therefore useful in cases of bronchorrhœa. Next to its action on the respiratory passages, its diuretic effects claim our attention. In his early researches, Lépine thought favourably of its diuretic power, and anticipated good services from the drug in chronic nephritis. His latest and more expansive observations, however, have convinced Lépine that no such therapeutic employment of terpene was to be thought of; and that the drug, on the

contrary, might, especially in Bright's disease, do considerable mischief, as it decreases the renal secretion, and even causes albuminuria and hæmaturia. The latter consequence may ensue after large doses, and is to be diagnosed not merely by simply inspecting the urine, but by examining the solid sediment for blood-corpuscles, or testing the urine with oil of turpentine and tincture of guaiac, when in cases where blood is present the characteristic blue colour will appear. In healthy individuals, however, no untoward symptoms are ever produced, as was demonstrated by a large number of conclusive experiments. Testing the physiological effects of a watery saturated solution on the blood of animals, Lépine obtained the following results:—

1. Hæmoglobinæmia, transitory, and without hæmoglobinuria, but with considerable polycholia.
2. Anuria for twenty-four hours following the introduction of the drug into the blood. The scanty quantity of urine secreted during another twenty-four hours looked like a watery infusion.
3. Small quantity of urea in a dark-coloured urine.

As to the action of terpine on the nervous system, Lépine found it to act quite the same as turpentine, though in one-third of the dose required of the latter. In neuralgia and hystero-epilepsy, the drug proved actually a success in the limited number of cases in which it was tried by Lépine. In cases of bronchitis it is best given in a watery (and slightly alcoholic) solution, and can be associated with any convenient syrup. In nervous affections it will often be required to combine with it an astringent, in order to avoid diarrhoea.

PERISCOPE OF MEDICINE, CLINICAL AND PATHOLOGICAL.

By G. A. GIBSON, M.D., and WILLIAM RUSSELL, M.D.

ACTIVE PULMONARY CONGESTION ACCOMPANYING CARDIAC DISEASE.—This condition is illustrated by the case of a man who had aortic aneurism and insufficiency, with a hypertrophied heart. One evening, on coming out of the theatre into the cold but dry air, he was suddenly seized with respiratory embarrassment accompanied by marked oppression. At the same time he developed a severe stitch in the left side. On auscultation there were fine crepitant *râles* over an area not larger than the palm of the hand. The bases of the lungs were free. Acute pneumonia would have been diagnosed were it not that the absence of concomitant fever excluded the idea. The signs disappeared in the course of a few days. He had similar attacks frequently; indeed, every time he went out in the evening without taking minute precautions he was similarly affected. The only treatment adopted in the attacks was dry cupping over the spot where the *râles* were present. It is specially to be noted that these active congestions appeared when the cardiac lesion was amply compen-

sated for. The expectoration varied in character, but was at times pneumonic or streaked with blood. It is maintained that this condition is to be clearly separated from the embarrassment of the pulmonary circulation so frequent in cardiac affections, and which are the result of stasis. The active congestions are essentially transient and usually localized. These attacks may be often unimportant in themselves if the cardiac lesion be amply compensated for, but they may be sufficient to overturn the balance and be the starting-point of the events which follow failure of compensation.—Le Dr M. Lebreton, *Gazette Médicale de Paris*, 31st Oct. 1885.

PTOMAINES OF CHOLERA.—Researches on the comma bacillus by Nicati and Rietsch were communicated to the Academy of Medicine in October and November. By the injection of pure cultures into the veins of an animal they had produced, by very small doses, the symptoms of cholera, ending in death. They had also separated from these cultures a liquid ptomain capable of producing similar effects. The authors had also separated from the blood and liver of cholera patients dying rapidly and in the algide stage, and when the autopsy had been made two hours after death, an alkaloid presenting the same chemical characters and the same action on animals as the alkaloid separated from the pure cultures. These facts, it is contended, tend to demonstrate that the comma bacillus is the pathogenic agent in cholera, and that this ptomain is the active element.—*Archives Gén. de Méd.*, December 1885.

TYPHOID FEVER, FOLLOWED BY FATAL PERICHONDritis OF THE CRICOID AND ARYTENOID CARTILAGES.—This case is reported from the service of Professor Villemin at Val-de-Grâce Hospital. The patient was 25 years of age, and never had syphilis. The attack of typhoid fever for which he was admitted ran an uneventful course, and convalescence seemed well established. He, however, developed signs of bronchiectasis in the left lung. Seven weeks after the onset of the fever pain in the throat was complained of, which was aggravated by coughing, and a fortnight later dysphagia and partial aphonia supervened. Laryngoscopic examination revealed a sessile polypus on the posterior part of the left inferior vocal cord, which was believed to be of tubercular origin. The propriety of surgical interference was considered, but before any steps had been taken, the patient died suddenly and unexpectedly. At the post-mortem examination the intestinal ulcers were found cicatrized. Several of the bronchi in the left lung presented bronchiectatic dilatations, and some of the bronchial glands were enlarged and tuberculous. The cricoid cartilage, in its entire circumference, was eroded and was bathed in pus, which lay within the perichondrium. The case, apart from the interest attached to the laryngeal affection, showed also the

association of bronchiectasis with tubercular glands, which is rare, and was found by Barthez only once in forty-one cases.—*Le Progrès Médical*, 17th October 1885.

THE FERMENTATION TEST IN THE DETERMINATION OF SUGAR IN THE URINE.—Einhorn has performed a long series of experiments with the fermentation test in Salkowski's laboratory, and from the results of his investigation he has drawn the following conclusions:—1. Urine affords better conditions for the process of fermentation than water, so that a solution of glucose in urine gives a little more carbonic acid than a solution in water of similar strength, and if the urine be concentrated the process is still more aided. 2. Additions of salts or other substances are not necessary in the application of the fermentation test. 3. Without any further check beyond taking equal quantities of yeast, it is possible to determine sugar when present in the proportion of $\frac{1}{2}$ per centum. 4. By boiling urine for ten minutes, it is possible to detect sugar when present in the proportion of $\frac{1}{2}$ per centum.—*Archiv für pathologische Anatomie und Physiologie und für klinische Medicin*, November 1885.

PERISCOPE OF OTOTOLOGY.

By Dr KIRK DUNCANSON, Senior Surgeon to the Eye, Ear, and Throat Hospital, 6 Cambridge Street; Lecturer on Diseases of the Ear, Edinburgh School of Medicine.

RECENT OBSERVATIONS RELATING TO MUMPS.—Dreyfuss adds two more cases, occurring in his own experience, to the thirteen cases described by Lemoine and Lannois, and the additional three published by Buck, of New York, of unilateral deafness following mumps. He shares the opinion that many cases of temporary deafness or impairment of hearing following mumps are overlooked by parents, and that probably many cases of unilateral deafness, not attributable to any direct cause, date their origin from a preceding attack of parotitis. According to him, aural troubles set in usually a few days after the onset of the main trouble; in both his cases, not till twelve days after recovery from a mild attack of mumps. Cases are recorded, however, where aural trouble preceded the attack of mumps by two to three days. The aural complications are generally ushered in by rushing, hissing tinnitus, and more or less pain in the ear. Later on, the train of Ménière's symptoms, such as vertigo, nausea, and vomiting, set in. Convulsions or loss of consciousness have, however, never been noticed. In both the author's cases the aural disorder was merely preceded by a feeling of malaise and lassitude. Deafness develops very rapidly, in a few hours, never later than within two or three days; while the membrane, pharynx, and Eustachian tube are to all appearances perfectly normal. The author does not share the opinion of Lemoine and Lannois, that deafness is always irre-

mediable, as hearing power was almost entirely restored in one of his cases. Dreyfuss entertains views similar to those of Brunner, that the pathological changes consist in an exudation into the labyrinth, with subsequent atrophy,—changes resembling those in the testicle in attacks of mumps. He rejects the assumption, as not being tenable, especially in those instances where aural complications arise prior to or after the general attack, that the process is an inflammatory extension from the parotid gland along the facial nerve or through the Glasserian fissure. Furthermore, the rapid development of the deafness would not favour this hypothesis. The author thinks that we have to deal here with a localization (metastasis) of the morbid products, independent of the usual limits of parotitis. Not only the lymphatic, salivary, and seminal glands, but also the organs of special sense are affected in parotitis. The disturbances in the auditory organ are analogous to those which have recently, from the same origin, been noticed in the visual organ, namely, conjunctivitis, amblyopia, and optic neuritis.—*Gazette hebdom. de m d. et de Chirurg.*, No. 30, 1884.

MUMPS AS A CAUSE OF SUDDEN DEAFNESS.—In addition to a general review of the whole subject, with abstracts of the already published cases, Dr Laertus Conner relates one case. The patient was a girl who, a month preceding the examination, had had mumps. As the attack was subsiding, she was suddenly seized with dizziness, staggering, nausea, and vomiting. Seen four days afterward, she was in bed on account of the nausea and giddiness. Temperature and pulse normal; tongue coated. After the action of some calomel powders, the nausea and dizziness disappeared, but a deafness in the right ear remained. When tested by Dr Conner, there was no perception of sound by the watch, tuning-fork, or Politzer's acoumeter. Inflation made no change. The drum-membrane indicated a past inflammation of the middle ear. Hearing in L., $\frac{36}{72}$ condition of membrane same as R. Pharyngitis. Examined four and a half months after, the hearing in L. was $\frac{40}{72}$: increased by inflation to $\frac{48}{72}$. R. still in same condition as regards watch, but the tuning-fork is heard on bone, and also at a distance of 4 inches from the ear; the acoumeter at 2 inches. The author thinks it probable that the disease extended from the parotid gland through the middle ear to the labyrinth.—*American Journal of Medical Science*, October 1884; Dr A. Hartmann of Berlin, and Swan M. Burnett of New York, U.S.A., in *Archives of Otology*, vol. xiv. Nos. 2 and 3, 1885.

IN a case of deafness brought to me, now more than ten years since, in a young woman with old scrofulous cicatrices on both sides of neck and chronic suppurative inflammation of both ears, with perforations and discharges from both; disease had done most injury to right ear. The cicatrices were more numerous

larger, and more recent on right side. The ear trouble dated back to early childhood, from scarlet fever. Extreme deafness had followed a very severe attack of mumps some six months previous to her visit to me. Her mother could recall no premonitory symptoms, such as are mentioned in the above notes; but from her account, her daughter's illness must have caused them much anxiety, and it was only after her return to her usual duties that they realized she was deaf. They thought she had not completely regained consciousness after her wandering and delirium during her illness. Syringing with a tepid alkaline solution removed from both ears, but most from right, a large quantity of gritty, purulent matter, which must have been there for some time, as the discharge had not been nearly so profuse since the attack of mumps. Subsequent treatment removed from tympanic cavities, chiefly from right, considerable masses of caseous, tubercular-looking matter, having all the appearance as if from scrofulous glands or lungs, even when examined and compared by the microscope. For some years the restoration of some amount of hearing, especially in left ear, was hoped for, as the tests were at times encouraging, and the improved condition of the ears cheered us on; but returns of scrofulous attacks either retarded or prevented what both patient and surgeon longed for, and now the patient employs sensation to appreciate sounds.—
Kirk Duncanson.

PROFESSOR DR JOS. GRUBER relates a case in the *Monatschrift für Ohrenheilkunde*, etc., for August 1885, where the upper two turns of the bony cochlea were thrown off through necrosis, the patient regained health, and was only partially deaf in the affected ear. Professor Gruber states that he knows of only two other cases which have been reported: one by Dr Cassells, the other by Dr Christinnek. The one reported by the late Dr Cassells of Glasgow drew my attention to this matter several years ago, and since the appearance of Professor Gruber's paper, I have had the opportunity of re-examining two cases where considerable portions of the bony internal ear, including partial windings of the cochlea, were exfoliated through necrosis, and removed after complete separation of the necrosed parts. Some of these specimens I have in my museum. Another case at present under treatment, the sequestrum has been removed, but the parts are not entirely healed; and in all three cases a certain amount of hearing remains in the ears, from which some of the bony parts of the internal ear have been removed. All of them can hear Politzer's acoumeter at 2 inches in front of that ear; one, the youngest, at 4 inches. Various vibrating tuning-forks are heard aërially and through bones. I may here mention that it was in the reduction and keeping down of polypi, which are so apt to arise during the separation and extrusion of these necroses that I in 1872

first used the spiritus vini rectificatissimus, Prus. phar., as suggested by Dr Gruber. Its beneficial action was mentioned at a meeting of the Medico-Chirurgical Society of Edinburgh, and its use has been taught and practised by me ever since. See report in *Edinburgh Medical Journal* of meeting of Medico-Chirurgical Society for 5th June 1872 (see *Edinburgh Medical Journal* for Nov. 1872).—*Kirk Duncanson.*

IN an interesting article on "the use of bichloride of mercury in otorrhœa" in *Archives of Otology*, vol. xiv., Nos. 2 and 3, we find Dr H. L. Ferguson, Dunedin, N.Z., stating very frankly his reasons for giving up the bichloride and returning to carbolic solution. He states:—"I am now using the carbolic, in which I always put some bicarbonate of soda, to raise the specific gravity of the fluid. I find that in ears with perforations of the drum-heads, where the Eustachian tube is free, before syringing, if water or plain carbolic solution is used, and passes down the tube, the tube is not, as a rule, free to Valsalva afterwards, owing to the swelling of the mucous membrane from absorption of the water. If, however, I raise the specific gravity of the solution with salt or sod. bicarb., the absorption does not take place, and the tube remains free. Of course the same action takes place in the cavity of the drum as in the tube, but I am not aware of any one having suggested this simple precaution." I have practised the above *precaution* for the last fifteen years, and taught it for the last ten; and in recent times have, to a great extent at least, like Dr Ferguson, given up the use of corrosive sublimate tepid solutions, using in preference, as I had done before using the perchloride, tepid solutions of equal quantities of boric acid or borax, bicarbonate and sulpho-carbolate of soda combined, to most thoroughly cleanse the ears or nostrils with, subsequently drying them completely with salicylic acid cotton-wool, before applying remedies either in the form of powders or of "drops." Dr Ferguson's is a valuable contribution to aural surgery.—*Kirk Duncanson.*

PERISCOPE OF SYPHILOLOGY.

By FRANCIS CADELL, F.R.C.S. Ed.

GONORRHŒA IN THE FEMALE.—In regard to the etiology of the disease, while the evidence is not absolutely complete, even those who are sceptical will admit that the probabilities are altogether in favour of a parasitic origin. Neisser in 1879 maintained such an origin. He was not, however, the first, since Donné in 1844 discovered a spore in urethral discharges which he named *Trichomonas vaginalis*. Salisbury, in 1868, discovered *Crypta gonorrhœica*; Jousseau, in 1872, discovered *Algue génitale*; and Hallier, in the same year, *Konotecium gonorrhœicum*. Bumm asserts not only that

the contagious property of this disease is located in the gonococcus, but that this microbe is distinguished from all others which are contained in the discharges from the genital tract, by its faculty of penetrating living tissues and its manner of multiplication. Widmark has also shown that in gonorrhoeal conjunctivitis in adults the gonococci show the same tendency to penetrate below the epithelium of the cornea. The identity of this disease with the ophthalmo-blenorrhoea of new-born infants has been shown by Sattler, Haab, and other competent ophthalmologists, the distinction between it and ordinary purulent catarrh of the eyes in new-born infants, consisting in the absence of gonococci in the latter and their constant presence in the former, from inoculation with gonorrhoeal virus. Kroner stated that, of 92 cases of ophthalmo-blenorrhoea neonatorum which he had seen, the gonococcus was found in 63. He also examined the vaginal secretions of 21 mothers in the eyes of whose infants gonococci had been found, and of 18 mothers in the eyes of whose infants no gonococci had been found. In the former group gonococci were found in all cases. In the latter the characteristic hives of gonococci were absent, though diplococci were found. Oppenheimer found that the incubation period of ophthalmo-blenorrhoea neonatorum and gonorrhoea was the same, and that it was most frequent in tedious, notably in so-called *dry* births. Negative results were obtained when the eyes of healthy infants were inoculated with lochial secretions in which no gonococci could be found. Welander found gonococci in the purulent urethral secretion of 79 women. In 25 of them he traced the source of contagion, and discovered gonococci in the urethral discharge of each of the 25 men. Positive results were obtained when matter containing gonococci from the urethra of a woman was applied to the healthy urethra of a man. The resulting discharge contained gonococci. Sternberg and two other gentlemen applied to their own urethrae, 23rd August 1884, cultivations of gonococci of the ninth generation. The results were negative, and tended to establish Sternberg's scepticism as to the infective properties of the gonococcus. The negative results of inoculation with the gonococcus, in any number of cases, the writer thinks, need not outweigh positive testimony in a single case, if all the conditions are reliable.—*New York Medical Journal*, 17th October 1885. (*To be concluded.*)

GONORRHOEA IN THE FEMALE. (*Concluded from page 501.*)—The author observed uterine gonorrhoea developing itself simultaneously with vaginitis on two or three occasions. In whatever manner the uterine gonorrhoea develops itself, it is important to remember how gonorrhoea persists in the uterus after it has disappeared from other parts of the generative organs. In the uterus, gonorrhoea takes up its position in the glandular *utricles* of the mucous membrane, as well as in the follicles of the urethra and its neighbourhood, and the vulva. This is most important as regards prognosis and treatment. Whilst

we look for the micrococcus in the uterine discharge, it is also necessary to seek for it in the follicles. Many facts regarding contagion hitherto inexplicable will thus be explained. The following are the appearances seen through the speculum in the acute stage of uterine gonorrhœa. The uterine orifice is intensely red, pouting, and tumefied, as is seen around the urinary meatus in the male when affected by a gonorrhœal urethritis. The mucous membrane is glazed or studded with little bloody erosions, more or less irregular, and resulting from epithelial macerations, thus differing from follicular erosions. They are erosions of the pavement epithelium, and are in no way the result of the gonorrhœal inflammation of the neck of the uterus. They are produced in the same way as the vaginal erosions, and the erosions of the vaginal portion of the cervix. These erosions sometimes unite to form patches, and in certain cases ulceration follows which, however, always remains superficial. When proceeding to the microscopic examination of this lesion, as Ruge has done, we find the chorion of the vaginal mucous membrane swollen and gorged with embryonic cells; this breaks by its rapid growth the epithelial lining, and erosions or superficial ulcers are the result. As the gonorrhœa advances, the uterine discharge becomes less abundant and the erosions disappear. Here and there little white irregular patches are observed, which gradually become of a uniform red colour. At this time there is a scanty purulent discharge from the uterine orifice, which is most marked when the cervix is compressed by the end of the speculum. These characters are to be seen when the uterine gonorrhœa is primary, and when the vaginal portion of the cervix has not yet become affected; they are even very recognisable when the uterine gonorrhœa is secondary, and when vaginitis is very intense. Along with the mucous erosions are seen small points, more or less ulcerated, of a bright red, sometimes white at the top, due to follicular uterine gonorrhœa in course of suppuration. In a patient now under treatment there exists an example of this lesion. Five or six prominent follicles, white and purulent, are situated at the orifice of the cervix and on the anterior lip. This patient, besides, is affected with a gonorrhœal urethro-vaginitis, and the peri-urethral follicles are also the seat of gonorrhœa. The presence of the micrococcus was established in the uterine follicles, as well as in the vaginal discharge and the peri-urethral follicles. The uterine cervix is hot to the touch, and the intrauterine temperature is raised from some tenths up to one and even two degrees. The uterus is sore to the touch, the size of the cervix slightly increased; the movements of the uterus are more or less free, according as the adeno-lymphitis is more or less developed or acute. The beginning of acute uterine gonorrhœa is sudden. The patients feel an extreme lassitude, with sharp fever, which confines them to bed. The other symptoms of acute metritis are present, as a rule. The menstrual function presents nothing special. The

menorrhagia noticed by some authors as the appanage of prostitutes affected with gonorrhoea the writer has never met with. It is the result of a material lesion, and not connected with a profession or a virulent affection. Acute uterine gonorrhoea is easily cured so long as the follicles are not affected. However, sometimes it becomes chronic, and resists treatment; it has returns of activity shown by a marked increase in the acuteness of the peri-uterine adeno-lymphitis. The duration of this metritis is very variable, and may last for days, weeks, or even years, during which time it remains more or less contagious. Gonorrhoeal metritis may pass into the chronic form either by localizing itself in the uterine glandules, or on account of some pre-existing constitutional malady, such as scrofula, gout, or rheumatism. Chronic uterine gonorrhoea is not in general marked by any functional trouble. Menstruation is regular, abdominal and lumbar pains are absent, and sexual intercourse is painless. Rollet has stated that women affected with this disease have great sexual desire. The writer's patients on being questioned have not given a satisfactory reply. Finally, those sympathetic troubles so frequently observed in virulent metritis are not met with.—*Annales Medico-Chirurgicales*, March 1885. L. Martineau.

Part Fifth.

MEDICAL NEWS.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentlemen passed their final examination for the qualification in Medicine at the sittings held on 3rd December 1885, and were duly admitted L.R.C.P. Ed.:—John Sutcliffe, Staleybridge; Herbert Leader Williams, Altrincham, Cheshire; John Broomfield Deaper, Manchester; Herbert Dunville Harthan, Macclesfield, Cheshire; Joseph Shuter Hill, London, S.W.

VACCINATION SHIELDS.—The medical officer of the Local Government Board occasionally hears of cases of erysipelas following vaccination, and traceable to the use of old and dirty "vaccination shields." If in any case, as where a dress is worn dyed with a possibly irritative dye, a vaccinator thinks some means of "protection" to a vaccinated arm to be desirable, he had best define the material and the manner of application of such appliance as he judges to be wanted in the particular case; and it appears to the medical officer important that every such appliance should be of a kind to be destroyed and replaced whenever it becomes soiled; and particularly that it should not be of a kind likely to be kept for subsequent use. The medical officer would, therefore, urge on vaccinators to discourage the use of the so-called "vaccination shields."

WE have received a very useful apparatus, the invention of a provincial practitioner, which deserves notice. It is a combination of stamp-box and stamp and envelope damper. By means of it the objectionable practice of licking the gum with the tongue is obviated, and the operation of affixing stamps and closing envelopes rendered pleasant. We heartily recommend the invention to the notice of our readers. It is to be had at Messrs Carlisle & Watts, Hanover Street, Edinburgh.

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etc., etc., *Professor of Materia Medica and Therapeutics, Catholic University Medical College; and Examiner in Materia Medica and Therapeutics, Royal University, writes in the "Medical Press," July 15th, 1885.*

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from the erectile tumour of the adult in the fineness and regularity of their meshwork. It will be found impossible with the utmost care to dissect out any distinct blood tube, on account of the constant intercommunication of one with the other. So perfect, in fact, is the reticulation that in whatever direction the section be made the lumina appear always to be cut transversely. There is no sign of a vessel running parallel to the section or cut obliquely.

The laminæ between the blood spaces are thin but tough, and smooth and shining on their surface. Here and there through the section denser white lines may be seen imperfectly dividing the tumour into lobes. It is not possible to separate between these lobules, and, indeed, they are often entirely absent. On microscopic examination, the laminæ are found to be fibrous in structure, lined by an epithelium, which is continuous with that of the bloodvessels.

By some recent authors these tumours are described as invariably surrounded by a distinct capsule, and on this point it is necessary to be perfectly clear, because on it has been founded a definite practice.

There are two modes by which a nævus may increase in bulk—the intrinsic and the extrinsic. It may grow by the invasion of surrounding parts, or by pushing them aside through addition to its own internal bulk. I have seen many a nævus run a course which entirely precluded the idea of its increase being otherwise than by involving the surrounding textures in its own mode of growth. Take one example. A child was born with a small mark on the temple exactly like a bruise, and which, indeed, was supposed to have been produced by the forceps used in delivery. But its true character was soon evident, for it grew with such rapidity that in a few weeks it covered one-half the face and palate, occupying the whole thickness of the cheek, greatly distorting the ear, and by enlargement of the eyelids entirely closing the eye. With surgical assistance, a process of involution was set up, and in two years not a trace remained in swelling, colour, or to touch, except a convergent strabismus from prolonged disuse of the eye. It is, of course, absolutely impossible that such a growth can have progressed otherwise than by increase or enlargement of the pre-existent capillaries of each part as it was successively attacked. In this mode of growth the formation of a capsule cannot take place. The tumour simply melts more or less abruptly into its surroundings. Nor is this by any means the least common mode of extension.

Again, the purely subcutaneous nævus is comparatively rare. The large majority of subcutaneous nævi involve the skin to a greater or less extent. Now, so far as the skin is concerned, capsulation is of course impossible. In the purely cutaneous there is no capsule whatever, and in the mixed the skin is perfectly continuous with the tumour.

But in many cases undoubtedly a capsule does exist, and there are plainly certain conditions which conduce to it. There is first the fact that sometimes the internal growth may be too rapid for the external invasion. We have in this respect a perfect analogy with the progress of malignant tumours. In the subcutaneous sarcomata, for example, there is often a wonderfully perfect encapsulation from this cause. So the nævi push aside so fast that they have not time to invade, or are so slow of invading that they cannot help pushing aside and condensing. Next we have a tendency to encapsulation from the natural anatomical relationships of parts. Here, again, we have an analogy in the behaviour of malignant tumours. They are slow to pass beyond the tissue or organ in which they began. They encapsule themselves, therefore, with regard to a new structure, while they extend rapidly in the old, whether it be bone, gland, or viscus. Referring, again, to the subcutaneous sarcomata, we find them for a comparatively long time easily separable from the subjacent fascia, and still longer from the subjacent muscle, while perfectly undefined in the skin and cellular tissue. So the nævus. It may become adherent, and may involve the fascia. The former occurs under pressure, the latter is very rare. But the anatomical separation, except when inflammatory adhesion arises, is usually distinct. You can strip them off the fascia by tearing, or with your finger. Yet, again, a capsule is formed when a subcutaneous or mixed nævus is old and retrograde. There is then no longer any tendency to invasion, and round a nævus, as round a bullet, the cellular tissue tends to condense.

So far, then, as the capsule is concerned, we find its presence to be very irregular. It is never to be found in the cutaneous portion, or in a nævus which is growing with rapidity, but the deep surface of the tumour is usually well defined, and if it be purely subcutaneous it is generally invested by a capsule throughout. I have said that many nævi melt insensibly into their surroundings. The margin, however, both in skin and deeper structures is usually pretty abrupt, whether it be encapsuled or not. Immediately beyond the area of capillary change the tissues are normal. The arteries which supply the growth are very small, but fairly numerous, and the veins, though larger in proportion, attain usually no great size.

But in the vascular arrangements there are many modifications corresponding to those of the cavernous tissue tumour. Thus it is not unusual for a congenital nævus to pulsate from the beginning; and I have dissected one of them in which it was possible to isolate several tortuous and dilated arterioles throughout the tumour. This may be called the arterial nævus. I have never, however, in the congenital erectile tumour seen any form which corresponds to the cirroid aneurism, or even to the aneurism by anastomosis of the adult. Similarly there is a venous nævus, and by the colour it is often possible to say whether, in a particular example, the blood

is more largely arterial or venous. When the skin is affected in the mixed variety, it is generally easy to trace in it the enlarged venous radicles running over the tumour. This is doubtless chiefly structural, and in the nature of the growth, but may also arise from the same cause which produces venous dilatation in the skin over any rapidly increasing tumour. But there is also a form of congenital nævus in which the very existence of the capillary metamorphosis is masked by the enormous dilatation of the veins. A most remarkable example of this form was exhibited by my colleague, Mr Joseph Bell, to the Medico-Chirurgical Society. The patient was a man of 27. He was, according to his own account, born with dark-blue marks on his left shoulder and arm, in which large veins were clearly noticed, and for this was taken to Dr M'Call Anderson when still a baby. There had been no change in the appearance of his arm, except a slight extension of the disease, within his recollection. He had fractured the left humerus when twelve years old, but the bone had united perfectly. The skin and subcutaneous tissue from below the left elbow to the shoulder, on the inner and anterior surface, and stretching across the root of the neck to the middle line, were occupied by a mass of tortuous and dilated veins. Mostly they were soft and compressible, with contractions and enlargements, much as is seen in varix of the leg; but here and there phlebolites could be felt, and the skin in many parts was distinctly nævoid. In parts also a firmer texture was to be distinguished subcutaneously, which, I think, could only be of cavernous structure. He had applied for relief on account of a swelling under the right sterno-mastoid, which had been coming and going for two years, and which probably was an extension of the growth. Its immense extent, its continuance into adult life, and the size of the veins, which in many parts were as large as the fore-finger, made it a most exceptional case.

2. *Pathology.*—The pathology of the disease is not altogether clear. Presumably the process of formation is somewhat as follows:—The capillaries dilate, and in the process of dilatation the intervascular tissue becomes compressed and atrophied. This atrophic action is shown in many ways, even if it might not be predicated *a priori*. Over a nævus on the scalp, for example, the hair grows sparsely, short, and thin; and in other parts the cutaneous follicles and glands in great part disappear. In some cases, indeed, this goes so far that the spontaneous or surgical cure of a nævus leaves behind a condition of skin somewhat scar-like in appearance. This atrophic, semi-inflammatory action advances to the destruction of such capillary walls as may be brought into immediate contact with one another, and so opens up communication between them. Such abnormal communications are known sometimes to open up even between large arteries and veins. It seems not unlikely that the cases of aneurismal varix

without traumatic origin are of this character. At least it is known that abnormal vascular communications may form in a quiet way and perfectly direct. It is, therefore, from analogy probable that the reticulation of a nævus is thus brought about. I have myself only had the opportunity of examining those in which the mesh-work was completely formed, and in which there remained no possibility of recognising the original capillary structure. This is not the case in the cutaneous forma. In them there is a pure multiplication and dilatation. If these extend to the mucous membrane of the mouth, it is easy to verify this fact by putting parts on the stretch. You have then displayed a most beautiful network of visible capillaries, often in parallel rows, with here and there a winding arteriole or more largely dilated vein. But other observers acknowledge the intermediate stages in all varieties. Billroth, for instance, describes the plexiform and cavernous. Recklinghausen, Paget, and others, while also pointing out that some nævi consist of a mass of tortuous and dilated capillaries and others of erectile tissue, explain that in the former there is multiplication, not merely dilatation of vessels. They describe this multiplication as taking place in two ways: by the extrusion of buds from the pre-existent vessels, and by the formation of new blood-paths in the tissues. Rindfleisch holds the process to be a cell infiltration, possibly inflammatory, of the connective tissue, which by subsequent contraction drags upon and dilates the vessels.

This last explanation of the pathology has difficulties in the way of its adoption. At the same time, the nævus has distinct relationships to inflammation. It is, for example, peculiarly prone to become inflamed under such causes of inflammation as friction or traumatism; and we are in the habit of utilizing this tendency for the purpose of cure. In the cutaneous form, again, which is specially apt to last into adult life, and so give time for such developments, instead of an atrophy of the intervacular textures, there is frequently an overgrowth. This, in some instances, assumes the form of fibrous nodules and outgrowths, while in others an association may be noted with another variety of congenital aberration, the hairy mole.

3. *Clinical History*.—Nævus is generally described as congenital. In some cases, indeed, it is so. But, as a rule, there is nothing visible at birth, or it may be only a slight stain resembling a bruise. It declares itself, however, in a few days, a fortnight at the latest; and it may be said that it does not commence after that period. It is very variable in its rate of growth. At first it may be exceedingly rapid. It may attain a very large size, but it does so early if at all. After the first few months of life its increase is generally proportional to that of the rest of the body, more often less than more. Frequently it remains permanently without increase.

There is always a great tendency to spontaneous disappearance. Certainly more than half are thus naturally cured. It is, indeed, rare to find in the adult a nævus of the subcutaneous or mixed variety. This no doubt is partly due to the interference of the surgeon, which is specially successful in these forms, but mainly arises from spontaneous involution, to which probably the surgeon's success may in part be attributed.

There are instances, however, in which the disease persists through life. The cutaneous variety, for example, the port-wine stain, as it is called, is exceedingly persistent. This is especially the case on the face, and notably when it is associated with moles or hypertrophies. It is unfortunate that it is precisely in these cases that the surgeon's art is least available, the persistence being partly a cause, partly a consequence, of that fact. But the subcutaneous and mixed varieties also sometimes, though rarely, fail to go away. I have already referred to Dr Bell's unique case of venous telangiectasis as a notable example. In my former paper I described subcutaneous nævi which had developed arterial connexions in adult life. It would seem that the subcutaneous tumour is prone to outlast adolescence in so far as it may be associated with arterial, venous, or, as we shall presently see, some forms of degenerative metamorphosis.

The cure is evidently brought about through a fibrous change. In a large number, I think, I can predict, with tolerable certainty, whether or not they are about to disappear. An advancing nævus is characterized by the vividness of its colouring, by its softness and compressibility. On the other hand, if it be about to retrograde it is distinctly firmer; and this induration is somewhat irregular and nodular, parts of the growth soft, parts hard. There is an inflammatory element in it, followed by gradual atrophy, produced no doubt by constriction of the vessels through fibrous contraction of the intervascular inflammatory products. I have already pointed out that the nævus is sensitive to inflammatory causes. Under friction from the clothing, for example, the cutaneous portion will sometimes entirely disappear, and even ulcerate. It is exceedingly common to see a nævus the margin of which is of a brilliant red and distinctly spreading, while the central part is of a dull livid tint or perfectly white. This is no doubt due in large measure to the fact that the more prominent portion is that which is most exposed to external sources of irritation. At the same time, a nævus cures from the centre habitually; and it almost looks as if, like other parts of the body, it had a definite life history, the older parts undergoing involution while the younger evolve.

There can be no doubt that the disappearance of a nævus is largely influenced by the period of life. It sounds paradoxical, but it is true, that the critical period of the first teething may lead either to increase or diminution; and the more severe the con-

stitutional disturbance, the more distinct are its effects upon the nævus. The second teething and puberty also are not without influence in conducing to atrophy of these growths. Perhaps, however, the rule is that, modified by these conditions and accidental circumstances, the nævus gradually fades under independent tendencies inherent in its own nature. In some cases injury or irritant, accidental or surgical, start the process; in some it seems entirely unprovoked; and there are instances of nævi which, after resisting many efforts to bring about their cure, have quietly obliterated themselves after these efforts have long ceased.

But other changes may occur than those already detailed. The atrophy may proceed irregularly, and portions of the tumour becoming blocked off from the circulation may form cysts. These cysts present contents, in the shape of altered blood, which differ little from those of cysts arising in consequence of blood extravasation. Perhaps this fluid is usually more thin and watery, and I have not found in any of them the deposit of fibrin round the walls which is seen in hæmatocele or extravasation into cellular tissue. They are most often multiple, of varying size, and are surrounded by a quantity of fibrous tissue, the cicatricial remains of the rest of the nævus. I am not sure that these cysts are always blocked off and subsequently dilated bloodvessels, because on one occasion I found a cyst whose contents were purely serous mingled with others containing a dark fluid. It is probable that in some cases at least they are simply an example of the cystic degeneration which may attack every form of tumour.

Probably the most frequent degeneration, however, is the fibrous. The nævus undergoes the fibrous change which I have described as the precedent to natural cure; but the subsequent atrophy and involution fails to take place, or does so only imperfectly. It forms a tumour, then, of no great density, but resembling rather a somewhat closely-packed connective tissue, more or less distinctly encapsuled.

Cases have been recorded in which a sarcomatous change has taken place. Considering the association of malignant disease with different forms of local irritation, it is impossible to deny the possibility of such an occurrence. It is certainly rare, and I have not met with an example in nævus unconnected with the pigmented or papillary mole.

4. *Etiology*.—We really know no more of the causation of nævi than we do of most forms of abnormal local growth. Some have been attributed to injury; but, independently of the *a priori* improbability, there is reason to believe that the mistake has arisen from the resemblance to a bruise which they sometimes present in their early state. Injury may modify them undoubtedly, by the production of inflammation, by changing a simple into a pulsating nævus through rupture of arterioles, and possibly in other ways, but I do not think that it can produce them.

There can be no doubt of their hereditary character. As might be expected in a growth plainly developmental in character, the tendency of the parent is in this respect often impressed on the offspring. I have known many illustrations.

The influence of maternal impression I cannot regard as in any case adequately proved. It is rare to have the suggestion made even by the mother; and, when it is, inquiry will in the greater number show it to have been an after-thought.

5. *Symptoms.*—The appearance of the cutaneous nævus will vary much. The name of "port-wine stain" indicates the prevailing tint with tolerable accuracy. But just as the colour of the lip or cheek varies in different individuals, so the nævus may present every tint from purple to brilliant red, though usually there is a considerable dash of blue. The blueness is greatly increased in many cases by cold, and in all may alter from time to time under the influence of temperature, exertion, and state of health. The network may be so fine that it is impossible to distinguish the separate vessels, and impossible also, by pressure or tension, to cause the discoloration entirely to disappear; but usually larger vessels of a venous hue may be recognised permeating the prevailing redder colour, and from them the blood may be easily expressed. The margin fades pretty abruptly into healthy skin, but it is common to see little peninsulas and islands of a lighter tint stretching out from the general mass. It is not raised above the surroundings except in those cases to which I have already referred, in which fibrous nodules are associated with it. The growth of hair over it varies much. When it occurs on the scalp the hair is much diminished in quantity, but on other parts of the body the fine down may be even more abundant than is natural; and sometimes, as I have already said, it presents the appearance of a hairy mole.

The cutaneous nævus has its special site upon the face. If it occur on the body it is usually of small extent, and shows symptoms of belonging rather to the mixed class, by tending to be somewhat elevated and to pass deeper. It has also the more brilliant hue of that class, although it is possible that the higher temperature of the covered parts may diminish the appearance of lividity.

The mixed variety is always elevated more or less above its surroundings: if small, as a rounded swelling; if large, with a somewhat nodulated outline. In some the only alteration of colour is a certain bluish tint which shines through the integument, but in the great majority the skin is thoroughly incorporated in the growth. Sometimes it shows extensive discoloration over the whole surface, sometimes simply meandering veinules; and a very frequent appearance is a margin of brilliant red, with a central part more or less of a dull whitish-blue.

It is capable of being diminished by pressure, when any irre-

gularity of its consistence becomes more manifest; and it shows its erectile character by increasing on exertion, or when the child cries. The mixed variety is also prone to occur about the head and face, but is much more common than the cutaneous on other portions of the body.

The purely subcutaneous nævus is rarely large, and presents all the characters of the mixed form except the coloration. It is decidedly rare.

There is a distinct absence of symmetry in all varieties of nævi. On the head and face the mixed and subcutaneous varieties often occupy the middle line. The nose is a common site, and I have three or four times seen the tip converted by a nævus into a blue unsightly knob. They are not very uncommonly multiple, but I have never seen two symmetrically placed. Sometimes on the face they run up to the middle line, and then abruptly stop; but, on the other hand, they also frequently cross it more or less extensively.

6. *Diagnosis*.—It is impossible to mistake those in which the cutaneous portion forms a prominent feature for any other form of disease. The mixed variety may be mistaken for meningocele and spina bifida, the subcutaneous or degenerated for fatty, cystic, and sarcomatous tumours.

Undoubtedly the most important diagnostic difficulty arises in cases of meningocele, and I have on several occasions had these meningeal tumours sent me as nævi. The skin over a meningocele is frequently in appearance indistinguishable from that over a nævus. In both it may be quite unaltered, but in both it may present the same bluish red hue from dilated bloodvessels; and although the meningocele is more prone to have a livid transparency, and to be traversed by large veins, yet even when these characters are marked it requires some practice to recognise them, because after all it is a matter of degree. The consistence of both is often very similar, and the degree of compressibility may be identical, varying according to the size of the orifice in meningocele, the rigidity of texture in nævus. The mere fact that the growth occurs in the middle line should always put one on guard. Nævi, it is true, are found in the middle line. I have seen them in every situation where a meningocele may be, from the root of the nose backwards to the occipital protuberance. Situation decides nothing. But the meningocele is seen nowhere else. In that line, therefore, lies the difficulty. Cohesion to the bone calls for additional care. If absolutely mobile it is a nævus. But often it is not easy to determine the mobility; and I have seen a nævus over the anterior fontanelle rigidly adherent to, and no doubt involving, the membrane as it did the skin. If on compression a neck-like connexion, and still more certainly if the contour of the connecting orifice can be felt, a very important point of distinction is made out. If a distinct history that it is not absolutely congenital can be obtained, it will go far to establish

its nævoid character; but the difficulty of obtaining a reliable history is proverbial. Should doubt still remain, the exploring needle may be harmlessly used in either tumour. If clear fluid be obtained it removes all doubt, but it must not be forgotten that a meningocele blocked off from the membrane may have its fluid reduced to trifling bulk by lymph deposit on its walls.

The same considerations apply to the diagnosis of spina bifida from nævus. I have twice had cervical meningeal tumour sent me as nævi; and on one occasion I had myself great difficulty in arriving at a decision in a case of spina bifida in the lumbar region. I inclined to consider it a nævus, so close was the resemblance in all characters; but in the end its nature was made clear by an alteration in consistence and relative position, which enabled one to distinguish the cleft in the spine. I had carefully abstained even from tapping because of the doubt, and because the consistence, firm and solid, suggested its natural disappearance in either case.

The congenital, or nearly congenital, origin, the shape, consistence, and situation, serve to distinguish a nævus, even when purely subcutaneous, from other tumours.

7. *Treatment*.—It cannot be too strongly insisted upon that no nævus should be submitted to treatment unless it be clearly increasing, or, as very rarely happens, doing harm by its presence. The reason for this lies in the fact that the majority of nævi disappear spontaneously, and that, on the whole, Nature's result is as good as any that can be produced by art. The justification for treatment, moreover, is not usually the danger of the disease, but its unsightly aspect. The danger attending the tumours comes from hæmorrhage, the result either of wound or ulceration. In the former case it bleeds very freely, nay, even dangerously, but this may be controlled, at least temporarily, by pressure; and I have known them heal, and even become permanently cured, after a cut of considerable size. Moreover, the chance of such an accident is very small in a young child during the period when it is still undetermined whether it will increase or decrease. If ulceration occur from friction of clothing or other cause, repeated small, and sometimes even severe, bleedings may take place; and the mere presence of ulceration, therefore, although it also will often heal, and even lead to spontaneous cure, I look upon as sufficient justification for interference. Again, it may be wise to apply treatment in such a case as this:—A child was sent me by Dr Sidey with a nævus, mainly subcutaneous, of the root of the nose. I was unable to ascertain that it was increasing, but I applied electrolysis, because the swelling was inducing a squint.

Putting aside, however, these cases in which there is risk or harm from its presence, to await the natural cure is the wiser plan in a tumour which is stationary. Expectancy is better and safer than interference.

On the other hand, it is right to apply treatment should the

nævus be enlarging. It is impossible to tell what size it may attain; and the larger a nævus, the more difficult is it to bring about a cure without risk and without scar.

The methods of treatment which have been employed are very numerous. I do not intend to discuss them all, but I shall endeavour to point out those which may be usefully applied under different circumstances. There is no method of universal applicability. One is better in one variety, another in another; and according to the situation and size, different means must be adopted. I shall discuss them in the following order:—

1. External application of escharotics or irritants.
2. Subcutaneous application of escharotics.
3. Ligature.
4. Excision.

I leave out of account such harmless placebos as the application of contractile collodion or other form of gentle pressure, which may be applied during the period of expectancy, and which may amuse the patient's friends, and possibly may sometimes aid the natural resolution.

1. The external application of escharotics or irritants.

There are three conditions under which this form of treatment may be adopted:—

a. On nose, cheek, or elsewhere, a tiny nævus is sometimes to be seen, not larger than the head of a pin, with little vessels radiating from it. It rarely increases; it usually goes away of itself. But it sometimes persists or grows; and its cure is so easy and simple, that I think it may be classed among those which ought to be treated whether they are growing or not. To dip a pin in strong nitric acid, and thrust it into the centre of the spot, is at once effectual. The same result may be produced by a heated wire, by a galvanic cautery point, or by the insertion of an electrolysis needle (the negative pole by preference), and the production of a little eschar thereby. The nitric acid or the heated needle are effective and simple.

b. There is a form of nævus which shows itself to be of the mixed variety by its slight elevation, but in which the subcutaneous part is exceedingly scanty. When this occurs on the covered portions of the body, it is frequently irritated by the clothes, and becomes obliterated in the centre. But at the margin there may remain a circle of brilliant red, which sometimes extends with considerable rapidity. In these cases I have been greatly pleased with the application of the sodium ethylate, as proposed by Dr Richardson, to the marginal zone. The liquor of the British Pharmacopœia is a good preparation. It is efficacious, not accompanied by much pain, and leaves a less conspicuous scar than most other powerful escharotics. If flexile collodion and salicylic wool be applied immediately, the eschar separates without suppuration, and very often a second application is not required.

c. Our only method of treating the port-wine stain is by means of external irritants. When it exists on parts of the body not exposed to view, it is better to leave it alone altogether. On the face it is so unsightly that an effort should be made to cure it. Unfortunately it cannot be said that our efforts are very likely to be successful. At one time I was inclined to give up the attempt, after trying multiple puncture, simple and with the cautery, Squire's multiple knife, and many other things. But as they so rarely disappear of themselves, and as I have latterly with persistence obtained somewhat better results, I believe we ought at least to give treatment a fair trial. It will require much patience, however, on the part both of patient and surgeon. Much time and care must be expended on them. The difficulty is to bring about a cure without destroying also the skin in which they reside, and so leaving a mark at least as disfiguring as the nævus itself. So far as I can see, our only chance is to bring about a dermatitis severe enough to lead to obliteration of the vessels, but not to produce ulceration. One attack of dermatitis, moreover, is rarely successful, unless the nævus be very small and pale. We have to repeat it over and over again through a period of many months; but looking to the effects of long continued frictional irritation, I have been encouraged to persevere, and, in some cases, have met with success. I have tried brushing with strong nitric acid, repeated blistering, iodine, perchloride of iron, and strong mercurial inunction. On the whole, I should say that iodine made the best application. The liniment, or the Edinburgh tincture, should be used and carried to strong irritation, to be repeated again and again as the cuticle peels off. The objection is the discoloration, so long continued and so conspicuous; but it is least likely to leave a permanent mark from its own action, and is at least as effectual as any other irritant.

2. Subcutaneous use of escharotics, coagulants, or irritants. This may be done in three ways:—

a. By the subcutaneous injection of such substances as tannin, carbolic acid, or perchloride of iron.

b. By the galvanic cautery.

c. By electrolysis.

I have given up entirely the first and second of these methods in favour of the third. The injection of coagulants is simply a coarser way of introducing under the skin substances similar to those more definitely and effectively introduced by electrolysis. The effect of this greater coarseness is, first, that it is difficult to draw the line between doing too much and doing too little, between causing a slough of the whole mass and leaving the tumour as it was. In the most cautious hands I have seen a destruction of tissue produced which gave rise to inconvenient scarring and deformity. Secondly, unless a preliminary temporary ligature be performed, injection is attended with some risk. Several cases of sudden death from the injection of coagulants have been recorded.

I have on two or three occasions inserted a platinum point or wire into the nævus, and, having then heated it by means of the electric current, have moved it about with the view of destroying subcutaneously the diseased texture. I do not find, however, that this method, as yet at least, possesses sufficient advantage in the way of precision or convenience to make me advise its use in preference to electrolysis, although it is not impossible that improvements in electrical apparatus and the mode of using them might entirely alter their relative merits, and even now in certain forms of erectile tumour the subcutaneous cautery may well be used.

The subcutaneous destruction of a nævus, then, is best done by electrolysis, and it only remains to be determined what cases are fitted for this treatment, and in what way it may be accomplished.

It is necessary, in the first place, clearly to understand what electrolysis actually does. In the *Lancet* for 5th December 1885 is published a paper on the treatment of urethral stricture by electrolysis, which I think is a good example of what a surgical essay ought not to be. The writer carefully abstains from describing his method of operating, suggests results which it is impossible to attain, and gives an entirely misleading idea of what he calls the "mysterious" action of electrolysis.

In a former paper in this Journal my friend Professor Fraser and I gave an account from experiment of the alterations in the blood which are produced by electrolysis, and in another I explained its action on the tissues of the body. It may be said shortly that the effect is twofold. There is first the primary electrolysis, whereby the blood and tissues are decomposed into simple constituents, the oxygen and acids appearing at the positive, the hydrogen and bases at the negative electrode. But, secondly, these nascent substances necessarily react on anything with which they have chemical affinity in their neighbourhood, whether that be electrode or tissue; so that, for example, the positive needle in a certain time may be entirely worn away. This secondary electrolysis gives their character to the sloughs produced. At the negative electrode the slough is diffuse, large, very slightly contractile, at the positive limited, small, and tending to condense in healing, the one corresponding to an alkaline, the other to an acid cauterization. Practically, then, in electrolysing a nævus we introduce subcutaneously so much caustic potash and nitric acid made out of the tissues on the spot. We introduce them in such infinitely minute division, and we so destroy tissue in producing them, that their action is purely local, and they cannot be carried into the circulation, while the small quantity produced is yet extremely potent, because it is nascent. By doing this subcutaneously we avoid the evils of putrefaction, and we conserve the skin. If properly performed, the effect is that, after a little tenderness, the slough is slowly absorbed without suppuration and without mark.

The cases in which electrolysis should be used are the mixed and subcutaneous varieties of nævus which are situated on exposed parts of the body, as the face or hands, and those situated elsewhere which may not be amenable to ligature or excision. This limitation has not been sufficiently defined by those who have since adopted electrolysis, although even on first introducing the method to the profession I deprecated its indiscriminate employment.

The advantages of the subcutaneous use of electrolysis in nævi on exposed parts are sufficiently evident. It cures the disease without leaving the slightest trace of its action. The little needle punctures entirely disappear as a prick from any other needle does, and the absorption of the diseased structure is accomplished without the slightest puckering or scar. In bringing about this result it is absolutely safe, and the limits of its action can be more precisely defined than in any other subcutaneous injection or operation.

I have now been operating on nævi by this method for twenty years, and I find that in that time I have subjected to electrolysis one hundred and thirty-three (completed cases), so that some idea of the value of the method may be obtained.

On an average three operations are required in each case, but greater experience has enabled me distinctly to diminish the number of sittings of late years, by doing more at each without damage to the skin, and by increasing the interval between them, to allow of more perfect absorption, and so bring out the full benefit of each operation.

I have not had one fatal result or even anxious symptom in any case. The only danger, indeed, is in the anæsthetic, without which it is unpleasant to work in children on account of the pain. Even in adults I find it necessary to use an anæsthetic. They describe the sensation as that of a severe burn, and have always demanded it at the second sitting if I have persuaded them to do without it at the first.

The final result has not been definitely ascertained in about ten per cent. of the cases. These, chiefly Infirmary cases, have not reported themselves. I am not aware of any in which there has been failure to cure.

In six operations I carried the action too far in my endeavour to get as near to the nævoid skin as I could, and so produced a cutaneous slough. In two of these I then resorted to other measures. The remainder were cured with more or less marking by cicatrix.

In nine operations, which include four of these six, some suppuration occurred. All of these nævi did ultimately perfectly well, but in all I now blame myself for not attending sufficiently to the aseptic condition of my needles and fingers, or for carrying the action too far.

In five cases some trace of the former disease remains. This comes from the fact that sometimes after the tumour is cured the nævoid condition of skin remains to a greater or less extent.

Experience of others leads me to believe that even this will probably disappear with the lapse of time. In some a certain whiteness or scar-like appearance of the skin which had been affected persists for a certain time, and I think will occasionally prove permanent.

These results give a general idea of the value of the method. By far the larger number of the nævi so treated were situated on the head and neck, for the reasons I have already detailed. One or two examples in addition to those which I have published already may be adduced to show its applicability to cases which could not otherwise have been subjected to treatment.

Thus, a little girl two years of age was brought to me with a nævus occupying the whole thickness of the cheek, extending backwards to the auricle, which it had enlarged in the upper part to the size of a walnut. It required seven operations to produce a cure, but the ultimate result was most satisfactory. Although the skin over the greater part of the swelling was much discoloured, it improved *pari passu* with the subcutaneous portion, and in the end presented very little even of that excessive whiteness to which I have before referred.

Nævi are frequent enough in the interior of the mouth. Usually they are extensions of external tumours through the thickness of the cheek, or by direct superficial continuity over the lips. They occur independently, however, and I have used electrolysis with success in the tongue, the soft palate, and the inner surface of the lower lip. In such cases a gag must be used, and it is necessary to attend carefully to the precautions for the prevention of bleeding.

Again, a child aged three years had a nævoid tumour of the foot. It involved the greater part of the outer margin, and penetrated from sole to dorsum between the fourth and fifth metatarsal bones. The growth entirely disappeared after four applications of electrolysis.

I know of no other method whereby such tumours as these could be cured without risk and without disfigurement.

The method of operating is exceedingly simple and easy, but requires some care and attention to detail to attain perfect results.

It is unnecessary nowadays to go into minutiae as to the effects of the different forms of electricity. After trying many batteries of constant current, I have reverted to the Bunsen or Smee, with four to six cells of large size. In the Infirmary, where it can be prepared by others, I use the Bunsen of four cells, as giving the largest amount of chemical work with the least tension. But in private the Smee's battery, with plates about four inches by six, and having six cells, is most convenient. It is less dirty, has only one fluid, and is equally effective. The ordinary medical batteries do fairly well if a sufficient number of cells be used. But their high tension is a disadvantage, and using them in surgical work only once in a way you are very apt to find them acting badly

when you require them, and have still further to increase the number of cells employed.

Before operating, the poles should be tested in saline water, and only used if the evolution of gas is copious and continuous.

The needles should be insulated with vulcanite. I know of no improvement on those originally introduced by Professor Fraser and myself. I have been recently told of a case in which the vulcanite covering fused during an operation. This can only have occurred because the needle in the centre had been reduced to extreme tenuity to receive the vulcanite, and the exposed points been allowed to touch in the tumour. The resistance of the thin portion of the needle must have been so great compared to other parts of the circuit as to cause it to become overheated. Such a combination is very unlikely to recur, and is easily avoided by trial before insertion. The length of the exposed point must correspond to the size of the nævus, varying from an eighth to three-quarters of an inch. For the sake of sharpness, steel is the best material; but the positive pole, if of steel, requires resharpening after each operation, because it is acted upon electrolytically.

Both poles should be introduced, because in that way you get most work in the shortest time. In small nævi they are best placed parallel and equi-distant from each other and from the sides of the tumour. In large nævi I am in the habit of moving them, more especially the negative, at intervals from place to place, and even of introducing them through new punctures. If left stationary, the amount of action rapidly diminishes after ten or fifteen minutes, on account of the slough with which they surround themselves.

It is necessary to watch very closely the growing induration round each needle. It increases slightly even after the needles are withdrawn, and the action must be stopped before the skin is involved.

If the needles be very slowly withdrawn while the battery is still working, so as to cauterize slightly their track, not a single drop of blood will flow. If this be not done, it is necessary to apply gentle pressure for a few minutes.

The punctures should then be covered with a little antiseptic wool and flexile collodion, and if there be the least suspicion that the action has been carried too far, and the skin cauterized, this covering should be extended over all.

In large nævi too much must not be done at one sitting or in one situation. The slough has to be absorbed, and it is better to establish several small sloughs at different parts than a great mass at one.

I find that, except in cases where I have been over-cautious and done too little, it is better not to repeat the operation under six weeks. This, however, is to be determined by the rapidity with which the induration may disappear, and by its effect on the growth as a

whole. A very small slough may determine involution over a very large area, and so long as that is in progress it should be left alone. Every year I have been tending to lengthen the intervals. Of course one meets with difficulties in this respect. Patients from the country are anxious to get home again. But I am sure it is better to let them go and come back, even though the interval be thereby much lengthened, than to hurry on another operation before you can tell the effect of the first.

Ligature.—I regard ligature as applicable to nævi of the mixed variety in which the skin is largely involved, and which are not situated on exposed portions of the body. It is preferable in such to its alternatives, electrolysis and excision, for several reasons. A scar is not of importance in these cases, and the frequent necessity for repeating the operation and the tediousness of the treatment render electrolysis unsuitable. On the other hand, when the skin is largely involved, either excision leaves as large a scar, or, if the skin which is affected be not removed, there is no certainty that its vascular condition will be cured. Since excision gives no advantage in the matter of scar and success in curing, there can be little doubt that ligature is, on the whole, further removed from risk of hæmorrhage and septic irritation, and should therefore be preferred.

Again, even on the exposed portions of the body there are cases in which I use ligature rather than caustic, electrolysis, or excision. There are a considerable number of nævi of the face in which the proportion which the subcutaneous bears to the cutaneous part is very small. If these be of no great size—not larger, say, than a threepenny piece—I apply the ligature. There is not room for the subcutaneous application of the electrolysis needles without destroying skin, and so leaving a scar. An external escharotic must be made to eat so deeply as inevitably to leave a disfiguring pit. If the tumour be excised, as much tissue must be removed as by ligature, and the resulting mark is even greater, because if the ligature be properly used the scar is reduced to very small proportions, which are generally exceeded by that of excision, plus the needful stitches.

It is exceedingly important that the method of applying the ligature be carefully considered. There are many modifications. First, it may be applied either subcutaneously or externally. I have several times tried the former in large nævi, both with simple cord and elastic thread, and have generally succeeded in bringing about a cure. But it is not always reliable; at least it completely failed with me in a large nævus of the buttock, in which I succeeded in dividing the base subcutaneously without producing one drop of pus. In the successful cases there was more or less suppuration. If suppurative inflammation be essential, the method stands self-condemned by its ineffective drainage and attendant risks. I cannot, therefore, recommend subcutaneous ligature in any case, and think it unnecessary to describe it or any of its modifications.

External ligature has been applied after separating up the skin, or

by leaving a portion of skin outside the ligature. If a partial excision be thus performed, there seems no good reason why the operation should not be completed by excision altogether.

The whole growth, with the affected skin, ought to be included. The most convenient method is to cross two needles under the base of the tumour, and then tie firmly under them. If the nævus be large, it may be tied in halves or thirds. It is of importance that antiseptic precautions be taken. If the silk and needles be carefully carbolized, and they and the tumour covered completely with a layer of antiseptic wool and flexile collodion, the whole mass drops off dry and shrivelled without suppuration, while the scar is much smaller and less conspicuous than if it be allowed to slough off in the usual way. To maintain this covering air-tight it is necessary, at least if the nævus be large, to apply fresh layers round the margin every few days.

Excision.—When a nævus is purely subcutaneous, or when the cutaneous part bears a small proportion to the subcutaneous, and when it is situated on the covered portion of the body, it is best to excise it. Although the purely subcutaneous nævus is not common, it is very frequent, especially in large growths, to find the skin vascular over perhaps only a third or less of the surface. If it be a large and prominent tumour, it is probably best to remove the diseased skin along with it. If a very large portion of such skin be allowed to remain, it is not always certain to be cured. I have even seen cutaneous extension of the nævus after the subcutaneous part had been removed. Still that is rare. After the nævus has been excised, the vascular skin usually cicatrizes perfectly, and slowly assumes a normal hue, and this is always the case if only a small portion be left.

I prefer excision in these cases, because by it you have at least a great probability of healing by first intention. There is no slough to separate, as with ligature. There is no repetition of the operation, as with electrolysis. In these cases, moreover, you may rely with some certainty on the presence of a capsule, which greatly facilitates removal. But even when the capsule is adherent or absent, there is no risk of hæmorrhage if you cut well beyond the margin of the growth, as in most instances it is easy to do.

But it is only in these cases that it should be used. Some may extend its area of usefulness over a part of the ground which I have assigned to ligature, but it can never in any case be justifiable to produce a scar on the face or neck, and perform formidable operations on account of a disease which in itself is practically without risk, which frequently disappears spontaneously, and which by other methods and with a little patience may be cured without a mark and with perfect safety.

II.—ON THE OPERATIVE TREATMENT OF POPLITEAL ANEURISM.

By THOMAS ANNANDALE, F.R.C.S. Ed., Regius Professor of Clinical Surgery in the University of Edinburgh.

(Read before the Edinburgh Medico-Chirurgical Society, 2nd December 1885.)

THERE are now few surgeons who, when called upon to treat a case of uncomplicated and limited popliteal aneurism, do not first try the treatment of it by some form of compression. My own experience leads me to prefer either digital compression or a modification of Reid's method in these cases. My two last cases in private practice were treated successfully in the following way. An Esmarch's bandage was applied to a point immediately below the aneurism, and then pressure was made by means of a horse-shoe tourniquet upon the femoral artery at the groin. From time to time this tourniquet was slackened slightly, so as to allow some blood to flow into the sac, and was again immediately tightened. This treatment was carried on for from two to three hours at a time, an elastic bandage being applied, but not so as to stop the circulation in the limb, after the tourniquet and Esmarch's bandage had been removed. An interesting point in both cases was that the patients were not confined wholly to bed during the treatment, but were allowed to lie on a sofa, and even to sit up with the limb resting upon a chair. Both cases were completely cured,—the one in two weeks, and the other in about three weeks. In the one case only one application of the tourniquet was employed, the patient having after the application an elastic bandage constantly round the limb, but not used so as to interfere with the circulation. In the second case two applications of the tourniquet, at an interval of a week, were required, and the use of the elastic bandage for two weeks after the second compression. It is a well-known fact that cases of popliteal aneurism are occasionally very easily cured, and it may be that my two cases belonged to this class; but, as they were both under treatment about the same time, I have thought a brief note of them might be interesting.

When compression fails to cure a popliteal aneurism, or when the case is one unsuitable for it, the treatment almost invariably suggested and practised is ligature of the femoral artery at the apex of Scarpa's triangle. Should ligature of the femoral artery fail to cure the disease, or should pulsation in the sac return and persist after this operation, the usual advice and practice is to try compression above the seat of ligature, or to try the flexion method; or, these failing, to tie the external iliac or common femoral arteries. Should these plans not succeed, there is, as Erichsen¹ (*Science and*

¹ I quote Erichsen because he is a deservedly high authority upon the subject, and his work on Surgery has been quite recently carefully revised and brought up to date.

Art of Surgery, 8th edition, vol. ii, page 130) remarks, only the choice between "amputation and opening the sac." He further says:—"Of these measures I should certainly prefer amputation, as offering the most favourable chance to the patient." Mr Erichsen continues:—"The operation of opening the sac, turning out its contents, and ligaturing the vessel supplying it, is in any circumstances a procedure fraught with the greatest danger to the patient, and full of difficulty to the surgeon, even when he knows in what situation to seek the feeding vessel."

There are certain local conditions of a popliteal aneurism which are recognised by all surgeons to render the case unsuitable for compression. Among the principal of these local conditions are:—

- (1.) Large and rapidly growing aneurisms.
- (2.) Diffused and ruptured aneurisms.
- (3.) Aneurisms tending to involve or involving the knee-joint.
- (4.) Inflamed and suppurating aneurisms.

In addition to these local conditions may be mentioned an unhealthy state of the arterial system.

In the first three of these conditions the common rule followed is to try ligature of the femoral artery, but most authorities qualify this opinion by stating that the ligature of the artery is a very uncertain treatment in these cases, and that amputation will not unfrequently be required. In aggravated examples of these conditions immediate amputation is advised by some, as the ligature of the femoral artery frequently tends to produce gangrene of the limb.

In the fourth condition amputation has generally been resorted to, although laying open the sac has been performed under these circumstances. Erichsen (*loc. cit.*) observes:—"Though this plan has been several times tried, I am not aware that by it the surgeon has ever succeeded in arresting the bleeding from a suppurating aneurismal sac."

The present state of opinion in regard to the treatment of popliteal aneurism may therefore be summed up as follows:—

- (1.) Compression in favourable cases, and when it can be borne.
- (2.) Ligature of the femoral artery when compression fails, or is unsuitable.
- (3.) Amputation when certain local conditions or complications exist.

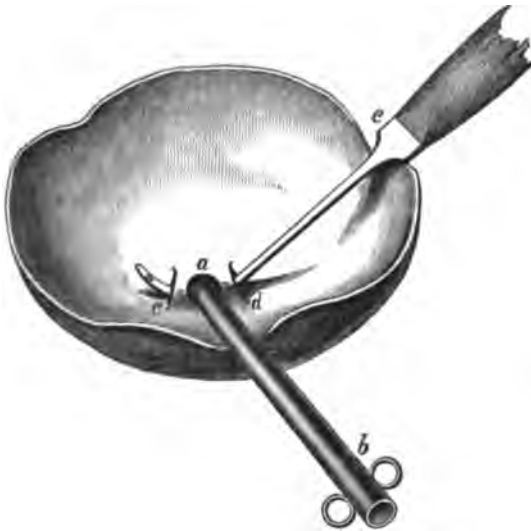
The "old" operation, or laying open the sac and securing the artery at its point of communication with it, is occasionally referred to by authors, but it is certainly never advocated in the case of popliteal aneurism, and in the passage quoted from Mr Erichsen the general opinion in regard to this proceeding is, I think, correctly expressed.

While, then, I advocate the treatment of popliteal aneurism by some form of compression in suitable cases, and this failing, by

ligature of the femoral artery, my object in this paper is to express the opinion that the "old" operation has hitherto been too much ignored by surgeons, and that in certain cases of this disease it will prove to be a more safe proceeding than those methods which are usually adopted.

It may be well to remark here that, before the introduction of the Hunterian ligature, popliteal aneurisms were not unfrequently treated by laying open the sac, but the success then of such a proceeding was very slight, and this is not to be wondered at when we consider what this operation was. It consisted in freely laying open the sac, and then stuffing its cavity with some dressing, or in some cases pushing a hot cautery into its interior. If any attempt was made to ligature the artery at the site of the aneurism, it was simply a dive with a needle and thread in the position of the vessel, with the result that, when such a ligature was passed and tied, it usually included vein, artery, and other structures.

The "old" operation at the present time is a very different proceeding, and, in my experience of it, which has not been small, I have failed to meet with those difficulties so graphically related by Mr Erichsen. With the antiseptic ligature and dressing, I now look upon this operation as a very simple proceeding in properly selected cases, provided you can stay the circulation in the sac during the operation, and you can always do this in a case of popliteal aneurism.



In order to explain my method of operating, I have had this diagrammatic sketch prepared. It is supposed to represent the cavity of the aneurismal sac after it has been laid open and all the clots removed. *a* is the opening of communication with the artery,

into which a bougie, *b*, has been inserted, and passed along the canal of the vessel upon its cardiac aspect. *c* and *d* are the two small incisions made through the wall of the sac, immediately above the opening, and the aneurism needle, *e*, is shown after its point has been passed through these incisions and under the artery, with the contained bougie. By means of the aneurism needle the ligature is drawn through and tied round the vessel upon the bougie, the latter being gradually withdrawn as the ligature is tightened. Should there be only one opening, and this is the case in a very large majority of instances, the same proceeding is carried out upon the distal end of the artery, the bougie being inserted into the opening again and passed downwards. If two openings exist they must be treated separately. The employment of the bougie was first suggested to me several years ago by Sir Joseph Lister, when I was operating upon a case of femoral aneurism.

✓ I will now relate a case in illustration. U. N., æt. 42, was sent to me in August of this year by Dr Hern, of Darlington, on account of a popliteal aneurism affecting the right leg. The patient had suffered from obscure pains, attributed to rheumatism, in this leg for one year and three months before his admission into my wards, but he only noticed a swelling in the popliteal region of this same leg about three months ago. Since first observed, the swelling, which pulsated strongly, has rapidly increased in size.

On examination, his condition of health was not very favourable. There was a slight systolic murmur over the mitral area, and the radial and temporal arteries were tortuous and affected with atheroma. The other organs were healthy. In the right popliteal space there was a large pulsating and expansile tumour, which filled the whole space, bulging out on each lateral aspect, but more particularly upon the inner side. A portion of the tumour felt firm and solid, but other portions were soft and fluctuating. A well-marked bruit was heard when the ear was placed over the aneurism, and the pulsation was especially marked over its centre. The leg below the knee was slightly swollen and cedematous, and the pulsation in the tibials at the ankle was very feeble.

└ After consideration, I decided to treat the case by the "old" operation, and upon the 2nd of September I made a small incision into the aneurism, the circulation of the limb being controlled by a tourniquet applied round the upper third of the thigh. Having introduced my finger into the wound, and by means of it loosened the adherent clot in the sac, I laid the whole sac freely open, and removed all the clots contained in it, and I then found that there were two openings communicating with the sac,—one corresponding to the upper end of the popliteal artery, just at its junction with the femoral, and the other to the lower end of the popliteal artery. Both of these openings were pervious, and admitted a No. 10 bougie, which passed freely into the canal of the artery. The sac of the aneurism was entire, and its inner surface was lined with many

layers of firm and laminated clot, and in addition there was a considerable amount of soft and recent clot in its cavity. The two openings in the sac were now separately secured, after the manner shown in the sketch. By means of the aneurism needle catgut ligatures, prepared with chromic acid, were applied, and when the tourniquet was removed there was no bleeding from the vessel. The external wound was now stitched and a drainage tube introduced. Irrigation with a solution of corrosive sublimate (1 in 2000) was employed during the operation, and the wound was dressed with corrosive sublimate wool. His progress was perfect. Before the 10th of September the drainage tube was removed; upon the 27th of September the wound was healed, except at one small spot, the site of the drainage tube, and the patient sat up in an arm-chair. Three days after this he was allowed to walk a little in the ward with crutches, an elastic bandage being applied round the limb as a support. Upon the 15th of October he was discharged cured. On the 3rd of November, Dr Hern, in a note to me in regard to another patient, writes:—"Norris is quite well, and at work."

My reasons for deciding upon the "old" operation in this case were—

- (1.) The unhealthy condition of the general arterial system.
- (2.) The large size of the aneurism, and its rapid growth.
- (3.) The œdema of the leg, and feeble condition of its circulation.

The case was quite unsuitable for pressure, and it belonged to that class of cases already referred to, in which ligature of the femoral artery is acknowledged to be not only uncertain as regards the cure of the aneurism, but to be attended with a considerable risk of gangrene of the limb, secondary hæmorrhage at the seat of ligature, a risk not now so great as formerly, or inflammation and suppuration of the sac.

It may be said, and has been said, that if the arterial system is unhealthy it is most likely that the artery in the region of the aneurism will be especially affected and unsuitable for ligature; but Mr Syme proved the fallacy of this in connexion with his brilliant operations after the old method; and having myself performed many operations of a similar kind, I can confirm his opinion, for I have never seen secondary hæmorrhage occur when the old operation has been carefully performed with anti-septic precautions.

In favour of the "old" operation in my case, was the certain and speedy cure and obliteration of the sac provided all went well; the immediate removal of the large tumour, which was pressing upon the veins, and probably also upon the arteries, and interfering with the proper circulation of the limb,—so that in this way two of the principal risks, gangrene of the limb and suppuration of the sac, were in great part done away with; and,

lastly, that if any of the risks already mentioned had followed my proceeding, and amputation been necessitated, an amputation could have been performed lower down in the thigh than in the case of ligature of the femoral artery, and therefore with less risk to the patient. But I have additional experience to support my opinion. In the *British Medical Journal* for 17th April 1880, I published the notes of a case of popliteal aneurism treated successfully by the same proceeding. This case was that of a man, æt. 36, who, seven years before he came under my care, had his femoral artery ligatured in Australia, after pressure and flexion had failed to cure the aneurism. The ligature of the artery was successful, and the disease apparently remained cured until a few weeks before he consulted me, when pulsation had returned, and the tumour steadily increased in size. In this case I laid open the sac, and secured the popliteal artery at the point of its communication with the sac. This patient was perfectly well in six weeks, and remains well. In some remarks upon this case I then wrote:—"The case is an additional proof that the popliteal artery may be successfully ligatured;" and again, "I have hopes that the successful result of this case may in the future cause it (the "old" operation) to be practised with more encouragement in some of those cases which have hitherto been treated by amputation." Further, I have proved that both popliteal artery and vein may be successfully ligatured; for in the *Lancet* for 24th April 1875, I reported a case of arterio-venous aneurism of the popliteal artery, the result of a wound, involving the artery and vein, in which I laid open the sac and secured both popliteal artery and vein at their points of communication with the sac. This patient was completely cured in five weeks. These are the only cases of popliteal aneurism in which I have performed the "old" operation, and therefore I have no unsuccessful cases as yet to record. From my experience of these three cases, and from my experience in other varieties of aneurism, I feel justified in expressing the opinion, that the "old" operation is to be preferred to ligature of the femoral artery in Scarpa's triangle in the following conditions of popliteal aneurism:—

(1.) In cases of large aneurism filling up the space, and interfering by pressure with the venous and other circulation of the limb below, or causing serious nerve pressure.

(2.) In rapidly growing aneurisms, which have attained some size.

(3.) In ruptured and diffused aneurisms.

(4.) In aneurisms which have involved the knee-joint by pressure.

(5.) In aneurisms attacked with inflammation and suppuration.

(6.) In aneurisms which the ligature of the femoral artery and compression have failed to cure.

(7.) In arterio-venous and other aneurisms of traumatic origin.

(8.) In cases of general arterial disease, provided surgical interference is considered necessary or advisable.

In such of these conditions, which are of an acute nature, there must be no delay in performing the operation; and I need scarcely add, that should symptoms of gangrene already be present in any case, amputation is the rule.

III. — CASE OF AMPUTATION AT HIP-JOINT, IN WHICH RE-INJECTION OF BLOOD WAS PERFORMED, AND RAPID RECOVERY TOOK PLACE.

By A. G. MILLER, M.D., F.R.C.S. Ed., Surgeon to the Royal Infirmary, and Lecturer on Surgery, Edinburgh.

(Read before the Medico-Chirurgical Society of Edinburgh, 6th January 1886.)

G. T., æt. 18. Admitted 5th December to Ward XVI., Royal Infirmary, with strumous disease of both hips, left knee and left elbow, and a large abscess connected with the left hip. He was very weak and anæmic.

History. — Had rheumatic fever eighteen months ago, also erysipelas. Was under Dr Duncan's care in Royal Infirmary nine months ago for disease of right hip.

Present Condition. — Right hip improving, but extension still necessary. Left knee and elbow affected with synovial degeneration. Left hip extensively diseased, a large abscess communicating with the joint.

The abscess was freely opened and drained on the 8th December, which gave some relief. But as the patient still suffered great pain, and there seemed no prospect of ultimate recovery, or even much improvement, amputation of the left leg at the hip-joint was suggested. This was agreed to by the parents and the lad himself, the risk from the operation having been fully explained.

Amputation was performed on the 18th December as follows (Dr Duncan having kindly undertaken the important part of collecting the blood that might flow during the operation and reinjecting it): —

An elastic bandage having been applied from the toes to the middle of the thigh, and a powerful elastic tourniquet at the groin, a rapid circular cut was made right down to the bone in the upper third of the thigh and the femur sawn through. A gush of blood took place, estimated at about four ounces, which was all caught by Dr Duncan in a vessel containing a solution of phosphate of soda. The femoral artery and some smaller vessels were then tied, and the tourniquet removed. After this a few more vessels required ligaturing, and a few ounces of blood escaped, which, however, Dr Duncan managed to collect and injected along with the previous quantity into the deep femoral vein. By an

incision on the outer side of the thigh the head of the femur was then dissected out. This part of the operation was accompanied by more bleeding than usual (five or six vessels requiring ligaturing) on account of the great vascularity consequent on the extensive amount of disease. The wound was thoroughly washed out with corrosive sublimate lotion, dusted with iodoform, brought together with four button sutures, and a few superficial ones, and the stump wrapped up in sublimated wool. After the operation the patient suffered from no shock whatever, nor had he any depression of temperature. For the first few days he was flushed, and had a fuller pulse than before the operation, but he had no rise of temperature. He has made an uninterruptedly good recovery, and is now (nearly three weeks after the operation) able to sit up in bed. The wound has always been dressed under protection of the spray, and is now quite healed as regards the deeper parts, there being only a superficial granulating surface along the line of the incisions.

The highest temperature recorded was 100·3. There was slight hæmaturia for two days. The day after the operation the number of his corpuscles was four and a half millions.

The case is one of special interest, as illustrating the advantage of Dr Duncan's method of blood injection which he described at a recent meeting of this Society.

The patient being in a very weak and anæmic condition before the operation, and the hæmorrhage during the operation having been greater than usual owing to the great vascularity of the parts from the extensive disease, it is very unlikely that he would have survived the shock of the operation had the greater part of the blood not been reinjected.

Dr Duncan, who watched the hæmorrhage, and measured the blood collected, and reinjected, calculates that the patient had an ultimate gain of blood after the operation. He estimates it thus:—

There was pressed back into the general circulation by the elastic bandage, say $\mathfrak{Z}v.$; reinjected of blood measured, $\mathfrak{Z}xj.$; lost in sponges and sawdust, say $\mathfrak{Z}iij.$; lost from destruction of corpuscles, say $\mathfrak{Z}j.$; net gain of blood, say $\mathfrak{Z}j.$ But to this must be added lymph from leg, say $\mathfrak{Z}v.$; solution, $\mathfrak{Z}iv.$ And also a diminished demand on the general circulation on account of the leg having been removed.

IV.—SUBJECTIVE SYMPTOMS IN EYE DISEASES.¹

By GEORGE A. BERRY, Ophthalmic Surgeon, Royal Infirmary, Edinburgh.

III. DIPLOPIA—POLYPOPIA.

WHEN double vision is complained of, we have, in the first place, to determine, by covering first one eye and then the other,

¹ Continued from p. 622.

whether in either case it is still present, or whether it always disappears when one eye alone is used. We have therefore to distinguish between binocular and monocular diplopia.

Binocular Diplopia.—In order that there should, under any circumstances, be binocular diplopia, it is necessary that the following three conditions should be complied with:—1. That there should be a fair amount of vision in both eyes; 2. That no circumstance should have led to a more than physiological suppression by the mind of the image falling on either eye; and 3. That both the visual axes should not be directed simultaneously on the object which engages the attention. (There is a curious possible though rare exception to this third condition, which will afterwards be referred to.) Thus there may be no diplopia complained of, even though there be wide divergence of the visual axes, owing to the suppression of the image of the one eye from more or less monocular blindness, or more frequently from habit. This is the most common condition in ordinary cases of concomitant squint. In such cases the suppression of the image of the one eye, when the other is used for fixation, is sometimes so complete that under no circumstances can it be made apparent; in other cases, by holding a red glass or a prism with the angle directed upwards or downwards in front of the fixing eye, the faulty image of the other is at once seen, showing that the suppression is only affected for normal conditions of similarity in the optical images, as well as for a retinal area only, on which, under ordinary circumstances, the images corresponding to those occupying the centre of the retina of the fixing eye are received. Again, an individual may complain of diplopia even although the visual axes are capable of crossing, and actually do cross on the same object. This is the case when other objects than the one fixed engage the attention, and is, in fact, physiological in so far as all objects not directly looked at are seen double. When, however, this kind of double vision is complained of, it is owing to an abnormal degree of attention being directed to objects other than those fixed. Usually there is a suppression of one of the images of other objects than the one on which the visual axes are directed,—a suppression which is often so complete as to render it difficult for many people to become conscious of this physiological diplopia. Which eye is the one whose images are suppressed in any particular case may be determined by a very simple experiment. By asking any one, while keeping both eyes open, to hold up their finger in a line with some distant object, and then close first the one and then the other eye, they will find that the finger exactly covers the object as seen by one eye, while it deviates to one side when looked at by the other. This shows that only the image of the eye in a line with which and the distant object the finger has been placed is observed, that of the other being more or less completely suppressed. The cases of diplopia now referred to are due to the opposite condition, viz., to a too

ready appreciation of the physiological double images. I have mostly met with this in women, but occasionally also in men. When once discovered by them, and not recognised as physiological, they contract a habit of directing attention to the double images, which sometimes becomes almost painful. Occasionally it is the nose which is in this way brought prominently before their notice, and which always appears to get in the way of other objects, by attracting attention at the same time; at other times, any objects both beyond and within the point of fixation are continually forcing their two images on the attention. This condition is by no means easily got rid of, even when the cause has been explained.

Patients who suffer from true diplopia, due to a misdirection of one of the visual axes on the object fixed, generally complain most of the doubling of that object, one image of which they usually recognise as the "true" image, that is, belonging to the properly directed eye, and the other as the "false" image, the image of the misdirected eye. The false image not only occupies a distinctly abnormal position, but is, besides, more or less indistinct, owing to its corresponding to a peripheral, and not a central, retinal impression. Although, however, we can infer from observing to which eye the true image belongs, which is the fixing eye, it does not by any means follow that the defect in muscular activity is to be found in the other, as the cause of diplopia may be a condition common to both eyes, or confined to the fixing eye alone, the eye made use of for fixation being often determined by causes altogether unconnected with the condition of which the diplopia is a manifestation. Occasionally, whilst recognising the doubling of the object fixed, the more distressing symptom complained of is a veiling of that object by the projection over it of the image falling on the macula of the misdirected eye, so that two different objects are constantly seen, the one through the other, as it were, just as the two images of a stereoscopic picture are projected to the same position in space. In cases where diplopia is complained of, it is often not constant, or it may be not present under all circumstances. This may be due to want of constancy in the conditions giving rise to it, as is the case in periodic squint, when the two visual axes are at certain times both directed on the same objects, whilst at other times only the one is perfectly directed; or there may be frequently suppression of the image of the misdirected eye. Again, in many cases of paralytic squint, especially where the paralysis is of recent occurrence, and not associated with contractions of antagonistic muscles, there is double vision only in certain directions of fixation. In periodic squint it is, indeed, greatly owing to the presence of diplopia during the period of squinting that the squint is not constant, efforts being made to overcome the muscular difficulties in order to avoid the disagreeableness of double vision, even although the alternative is often asthenopia. In ordinary cases of concomitant squint with

diplopia, fortunately a not very common complication, there is double vision over the whole extent of the field of fixation, the degree of separation of the double images being much the same, though not exactly the same, for all positions of objects equidistant from the eye.

In cases where there is a defect of convergent power, or an increased tendency to or spasm of convergence, there is often diplopia within or beyond a certain distance from the eye, a distance which not only varies according to the degree of the abnormality, but also according to the position of the eye above or below the horizontal plane of fixation. Thus any weakness of convergence will be more readily felt when the eyes are directed upwards, and will therefore give rise to diplopia for objects further away from the eyes than would be the case for fixation in the horizontal plane; whereas, when the eyes are directed downwards, the defect will be less appreciable, and diplopia only evident on approaching the object nearer to the eyes than required under similar circumstances to produce double vision for fixation in the horizontal plane. On the other hand, when there is either a nervous spasm of convergence, a very rare condition, or an acquired preponderance of convergent over divergent movements, as in some cases of myopia, the exact opposite is the case, that is, the abnormal conditions are most marked when the eyes are directed downwards, and least so when directed upwards, so that there is under such circumstances a greater range through which the diplopia is manifest for the depressed than for the elevated plane of fixation. This is owing to the circumstance that there is a greater tendency towards convergence on looking downwards than on looking upwards. Thus, while it is possible for many to diverge their visual axes on looking upwards so as to see distant objects, such as stars, in crossed double images, this is not only impossible when the eyes are directed downwards, but in that position there is even a much greater reluctance in relaxing the degree of convergence assumed for seeing objects near the eyes, so as to overcome, by a divergent movement, the diplopia which a prism with the angle outwards held in front of one eye causes. It is probable that these differences are the result of the requirements of vision; objects when fixed considerably below the horizontal plane through the eyes being usually near, and therefore necessitating convergence, those above that plane, on the other hand, being mostly distant. It is customary, in making examinations with a view of determining the relative positions of the double images in cases of diplopia, to select nine positions for that purpose, five of which are called primary, and four secondary positions. The primary are those into which the eyes are brought by purely lateral or vertical movements, whilst the secondary positions are such as result from movements which are combinations of these two. These combined movements cause the vertical and horizontal meridians of

the eye to be rotated outwards or inwards; in other words, the movements bringing about the secondary positions (upwards and inwards, upwards and outwards, downwards and inwards, downwards and outwards) are associated with a torsion or twisting of the eyes round their antero-posterior axes. The tests are generally made with a lighted candle, held first in the horizontal plane, at a distance of six to eight feet straight out from the face, then on the same level some distance to either side, then in the middle line above and below, and finally above and below to either side. The examination is facilitated by holding a red glass in front of one eye, as this not only makes the double image more apparent to the patient, but lets one know at once to which eye each image belongs. In this way important information is gained as to the nature of a parietic affection, which is often revealed by the diplopia before it could well be detected by observations based on objective methods of examination. In paralytic squints, the positions for which the diplopia exists are those into which an attempt is made by the weakened muscle to bring the affected eye in association with the other eye, so that the separation of the double images is greater according as such positions of the object seen double are more and more in the direction of the action of such muscle, and, of course, also in proportion as the paresis is more and more complete. An exception to this rule is found in cases where there is a paresis in both eyes of two or more muscles used in associated movements. Besides the differences in the relative separation of the double image which are found to exist for these different positions in the various forms of paresis, there is a difference in some cases due to the greater or less tendency to convergence, according to the degree of elevation of the eyes already referred to. Thus, in cases of paresis of the sixth nerve, we find, owing to this circumstance, that the line of demarcation between single and double vision, instead of being vertical, passes obliquely from the side of the affected eye above to that of the sound one below.

When the image seen to the right belongs to the right eye, or that seen to the left to the left eye, the diplopia is said to be homonymous; on the other hand, when the right image is that of the left eye, and the left that of the right, the diplopia is crossed. Homonymous diplopia is present when there is relatively too great convergence of the visual axes compared to the distance of the object fixed; crossed when there is relatively too great divergence.

Homonymous diplopia is met with in paresis or paralysis of the sixth and fourth nerves, and in strabismus convergens, also in the rare cases of spasm of convergence. In a large proportion of cases of convergent strabismus, the suppression of the image of the squinting eye is so complete as to exclude diplopia, but there is always diplopia in cases where the strabismus has come on suddenly, or in adult life, as when associated with myopia, and sometimes hypermetropia, or when due to any cause at the same time unconnected with a loss of distinct vision in the one eye. The

diplopia arising from convergent strabismus met with in myopia is, as has been already said, often only present for objects beyond a certain distance from the eyes, a distance which tends to become closer and closer in the course of time, and in this respect it differs from ordinary cases of hypermetropic strabismus, where, when there is diplopia, it exists for all distances. Occasionally one sees cases of hypermetropic squint, however, with diplopia, presenting exactly the same characteristics as that associated usually with myopia, where distinct distant vision is impossible for any time, at any rate without diplopia, whereas there is always single vision within a certain distance. These cases are at once cured by the use of correcting glasses, whereas it is only at the outset that the corresponding myopic strabismus can be overcome without tenotomy of the internal rectus muscle, an operation which, in such cases, is followed by its most brilliant results. Sudden convergent strabismus, owing to the assumption by the eyes of their position of rest or muscular equilibrium, is occasionally met with as a result of central changes leading to diminution in the breadth of fusion,¹ and always gives rise to homonymous diplopia.

Crossed diplopia occurs in paresis or paralysis of the third nerve, and in some cases of divergent strabismus, or within a certain distance in paresis or insufficiency of convergence.

If we fix a distant point, we observe that the crossed double images of a nearer object are further and further apart the nearer the latter is to the eyes, and similarly the homonymous double images of a distant object separate more and more the nearer the object is approached on which the eyes are fixed. There is, in fact, a definite angular separation of the two images corresponding to the distance from the macula of each eye on which the retinal impression takes place, although the distance of apparent or linear separation depends on the position of the plane on which the two images are mentally projected. We might, therefore, expect that it would be easy to calculate the angular deviation of the visual axes under any circumstances from a determination of the distances separating the double images. This is, however, not the case, the reason being that the direction of the outward projection of images formed on the macula of the eye depends apparently on the knowledge we have of the position of our eyes gained by the constant experience of the effort required to bring about that position, so that when a muscle becomes suddenly weakened, and a greater effort has to be made to attain, or endeavour to attain, some particular position, this increased effort is associated with a displacement of the line of projection in the direction of the action of the weakened muscle. Hence it is that, as the projection of peripheral images is always the same relatively to that of the central ones, there is often found, in cases of paresis, to be a greater separation of the double images than corresponds to the deviation of the visual axes. It is this abnormal projection of the images of the paralyzed eye which gives rise to the giddiness and

¹ See Pain.

occasional unsteadiness of gait, or other similar difficulties, experienced by patients with diplopia due to paralysis.

Another circumstance of a different nature may also produce the effect of causing the separation of the double images to differ in extent from that which the position of the visual axes would seem to demand. Thus it sometimes happens that after operations for convergent strabismus, although there may be no faulty position remaining, or even although slight convergence is still left, there is a spontaneous complaint of diplopia, or diplopia may easily be elicited by holding a red glass in front of one eye, when the double images are found to be crossed instead of homonymous. In such cases there must have been in the previous position of the eye a projection of the faulty image on or towards the point fixed, and therefore of the image falling on the macula lutea inwards, *i.e.*, to the opposite side of that point, whereas more commonly, notwithstanding the misdirection of an eye, the image of any point falling on the macula lutea is projected to the same place in space as that occupied by the object fixed, though, as we have already seen, there is only occasionally a consciousness of this projection. A new visual axis is, in fact, acquired, which passes through some peripheral point of the retina occupied by the image in the misdirected eye of the object fixed by, and therefore forming an image on the macula lutea of the other, so that the correction effected by the operation, producing as it does an outward displacement of this new visual axis, introduces a condition corresponding to divergent squint. This abnormal cause of diplopia does not, however, last long, as a new point on the retina soon becomes identified with the macula of the fixing eye, or the suppression of the image falling on the misdirected eye soon becomes complete. In this way it may happen that although the centres of each eye are directed on the same object there may yet be diplopia. This is the exception referred to as rendering the third condition for diplopia not universally true.

The directions in which the faulty image may be displaced are of three kinds:—1. Purely vertical or lateral. 2. Sloping. 3. In its apparent distance from the eye. The lateral and vertical displacements are the direct consequence of the imperfection in association of the lateral and vertical movements of the two eyes. They are therefore met with in all the positions in which these associated actions come into play. The sloping displacement, in which the faulty or the true image appears to slope towards or from the other, is due, in the case of paresis of the rectus externus or rectus internus, to the imperfection in the association of movements necessary to attain the secondary positions, in the case of the isolated paresis of other muscles, to the absence as well of their co-operation in the vertical movements, which can only be properly effected by the combined action of a straight and oblique muscle. The difference in the positions of the vertical and horizontal meridians of the two

retinæ, brought about by efforts to attain these positions under the abnormal circumstances, causes the apparent want of parallelism between the images. Owing, however, to the secondary positions being less natural and more constrained positions, there is not the same degree of precision in the association of the two eyes ever acquired in attaining them as is found for the primary positions. There is, consequently, when one muscle which aids under normal conditions in bringing about a secondary position is weakened, a position of the false image which corresponds not only to the defective action of that muscle, but also to the incomplete attempts at associated action of the remaining muscle or muscles, which results in bringing about a degree of apparent sloping of the image which does not always correspond in amount, and is generally greater than that which, from the conditions of physiological action, might be expected. The third displacement, that which causes the false image to appear nearer or further away from the eye than the true one, is one depending upon the position in space to which it is projected, and on this account is by no means invariably the same, even in the same case. The monocular estimation of distance is very imperfect, and easily affected by many circumstances, but there seems to be a tendency for images falling on the retina above the macula lutea to be referred to nearer distances, while those occupying positions below the macula are more readily projected to greater distances. This is probably due to the experience gained under the ordinary conditions of binocular vision. If we fix any object on the ground, the images of nearer objects are found on higher points of the retina. But it is impossible to draw conclusions as to monocular projection from what obtains binocularly, where the conditions for the estimation of distance are so much more favourable. Still we may, by simply holding a prism with the angle downwards in front of one eye, convince ourselves that the image of that eye is nearer to us than that of the higher one belonging to the other eye, provided always that the object fixed does not lie on a vertical plane, or on one sloping away from the eyes, otherwise its image being projected on a plane in such a position appears to be more distant.

A description of the relative positions of the double images in paralysis and paresis of the different muscles alone, or in various combinations, does not form part of the object of this sketch of diplopia as a symptom of ocular disorder; such a description is to be found in most text-books on ophthalmology. I have met with isolated pareses of all the muscles except the inferior oblique, and with many different forms of combined paresis, some of which require a good deal of consideration before the correct diagnosis can be arrived at. The more common cases of paresis of the sixth, third, and fourth nerves alone should be recognised without much difficulty by means of the diplopia to which they give rise. It is a good plan to make a chart for one's self of every case treated, giving the

patient a red glass in front of one eye, in order to make sure to which eye each image belongs.

So far we have seen under what conditions binocular diplopia is likely to occur, and how, by studying the relative positions occupied by the double images for different directions of fixation, it is possible to diagnose the nature of the defect in the muscular apparatus of the eye. As such lesions are often the result of cerebral changes, and may exist along with, precede, or follow other symptoms due to the same cause, it is evident that a correct diagnosis of the nature of the ocular paresis may be of great importance in enabling one to localize or even form some idea of the nature of the cerebral change. We have seen also under what circumstances conditions, which might give rise to diplopia, are not accompanied by that symptom, but there is one important development of the subject which yet remains to be considered. Thus there may be abnormalities connected with the muscles of the eye which do not give rise to any manifest diplopia. This is the case when they are of such a nature, or are so slight as to be overcome by the increased impulse which a strong desire to maintain binocular vision brings about, that is, by the power of fusing the images of the two eyes. The defect may be either of a paretic nature, often the last remains of a paresis in the act of disappearing, or it may be of the nature of a muscular insufficiency, from some causes already referred to¹ as giving rise to this condition. In either case some degree of asthenopia is apt to result from the efforts made to overcome the difficulty. The method by which a defect of this nature can be detected is both simple and elegant; all that is necessary is to hold a prism of 4° or more with the angle directed upwards or downwards in front of one eye; this causes a vertical displacement of the images of the two eyes, which, as they cannot be brought to overlap each other, causes the patient to relinquish any effort he may previously have exerted to get rid of double vision, and to allow his eyes to assume a position which is more nearly one of equilibrium. In consequence of this he may observe, besides the vertical displacement, a lateral or torsional one, due to the paresis or insufficiency which is otherwise concealed. By examining the relative positions of the double images thus elicited for different positions of fixation, the nature of the muscular defect may be detected. The question of the range through which the power of fusion of double images into one exists, i.e., of the breadth of fusion, is one of importance, as often affording a clue to the locality of the lesion, giving rise to some forms of paresis. Thus, if we find that when only a very trifling defect exists, causing but a slight separation in the double images, there is no tendency to overcome the diplopia; or if, for instance, in any case a prism whose optical effect counteracts that produced by the position of the eye only does so for one position, the diplopia appearing again as soon as the

¹ See Pain.

eyes are moved more or less slightly out of that position in the direction of action of the weakened muscle, there is evidence of a diminution, or it may be an abolition of the power of fusion. This would indicate, provided always that the case was not one of complete paralysis, and that there was good reason to infer that a normal degree of such power existed previously, that the cause of the paresis was central, i.e., not only due to cerebral lesion, but caused by a lesion involving the more central regions of the brain. Just as the use of a prism in the manner above indicated enables one to detect an existing insufficiency, what is called a latent or dynamic deviation, so in cases of paresis such a latent deviation becomes manifest, and influences the positions of the characteristic double images; for instance, the slight homonymous lateral displacement which should exist along with the vertical and torsional displacements in a case of paresis of the superior oblique may fail to make its appearance, or be replaced by crossed diplopia, when there is any considerable degree of latent divergence. Another circumstance which interferes with the arrangement of the double images which, in accordance with the physiological action of certain muscles, should characterize their paralysis, is the secondary contraction which often takes place, generally only after some time, in the antagonist. Thus the diplopia in a case of paralysis of the sixth nerve may not be confined to the side of the eye affected, but stretch to a greater or less distance to the other side as well, owing to a secondary contraction of the internus. These two conditions often add not inconsiderably to the difficulty of making a correct diagnosis.

Monocular Diplopia.—This may be only a variety of monocular polyopia—that is, it may be due to conditions which give rise to the formation of a number of separate retinal images, two of which irregular images are, however, more distinct and prominent than the rest; or the diplopia may be due to some cause which produces a mere doubling of the retinal image; or again, in some rare instances, it may result from an acquired faulty projection. The diplopia may exist even when the eye is correctly accommodated for the distance at which the object which appears doubled is situated, but the conditions giving rise to it are usually more likely to assert themselves when the eye is at the same time incorrectly focused for that distance. When suddenly complained of, it may either be due to some trauma, or the conditions causing it may have existed previously, and only be observed owing to some circumstance intervening which causes the attention to be directed to it. Of traumata likely to give rise to sudden monocular diplopia or polyopia may be mentioned the piercing of the lens with a sharp instrument, the partial rupture of the ligament of the lens (zonule of Zinn), or paralysis of some portion of the ciliary muscle resulting in its irregular contraction. Or there may be dislocation of the lens, or separation of a portion of the peripheral attachment of the iris (indo-dialysis). Again, the trauma may be of the nature of an operation, the diplopia being the more

immediate result of an extraction of a cataract, or following the operation of iridectomy for corneal opacity or synechiæ after iritis. Perhaps the most common causes of monocular polyopia are the changes sometimes occurring in the lens during the formation of cataract and the alterations of curvature of the cornea produced by inflammatory changes. It is also met with in cases of irregular astigmatism, and thus the existence of the diplopia can be verified by ophthalmoscopic examination, a doubling of the vessels being particularly noticeable in this way.

The defect of monochromatic aberration common to all refracting surfaces which are not of such a form as to neutralize it, and therefore called aplanatic, and which in optical instruments is practically to a great extent overcome by a centered system of surfaces of different curvature and different intervening distances, is to a great extent also rectified in the eye, partly owing to the dioptric arrangements, and partly, no doubt, also to the isolation of each percipient element of the retina. But there is a form of monochromatic aberration in the eye which does not exist in properly constructed optical instruments, which gives rise under certain conditions to polyopia. This aberration is due to the circumstance that the foci for rays passing through different portions (sectors) of the lens do not all coincide. When the eye is properly accommodated the want of coincidence of the foci is not sufficient to give rise to separate images, the only effect of the condition being that the retinal image of a point is larger than it would otherwise be. If, however, the retina be out of focus, the different images, instead of partially overlapping, are seen more or less distinctly as separate images. These images are crossed if the retina lies in front of the focal plane, and homonymous if it lies behind that plane; that is to say, that on partially occluding the pupil the images of the opposite or of the same side disappear. This kind of polyopia is very evident in cases of faulty refraction (ametropia), when a bright object for which the eye cannot be accommodated is fixed, and it is an exaggeration of this phenomenon which is the cause of the polyopia occurring in commencing cataract.

When not due to a trauma, the doubling of the image in one eye hitherto existing, though unobserved, may sometimes suddenly force itself upon the attention, either owing to increased retinal susceptibility to impressions, or to some cause having temporarily disabled the other eye, and when once discovered it may become more or less troublesome. In this way we sometimes find monocular existing along with binocular diplopia, the former being first brought to notice by the occurrence of the latter. When diplopia exists in a case of dislocation of the lens the patient generally sees, for near objects of fixation at any rate, one distinct and one indistinct image, the more distinct one resulting from the focusing of those rays which pass through the portion of the lens opposite part of the pupil, while the indistinct one arises from the less complete convergence on the retina of the rays passing by the side of

the lens. The relative positions of these two images differ in different cases according to the direction of dislocation owing to the prismatic effect of the edge of the lens. The diplopia in these cases of dislocated lens is one of the points which has to be taken into consideration in selecting the nature of optical correction best suited for improving vision; that is to say, in determining whether it is advisable to give concave glasses for the correction of the myopia produced by the displaced lens, or convex glasses for the correction of the hypermetropia resulting from the partial absence of the lens, as it sometimes happens that a glass otherwise suitable is of little use, owing to its rendering the diplopia more apparent and distressing; and this, again, is probably in no instance the case with both convex and concave glasses. The question of the occurrence of diplopia owing to a doubling of the pupil has sometimes a practical importance in connexion with the operation of iridectomy. On looking at a needle held vertically through two small holes some millimetres apart pierced horizontally in a card held immediately in front of the pupil, two images of the needle are seen if the eye be incorrectly accommodated for the distance at which it is held. This is the well-known experiment of Scheiner, and we may occasionally have somewhat similar conditions occurring in the eye as the result of disease or accident, the screen intervening between two pupillary apertures being either an opacity of the cornea or lens, or a portion of the iris tissue. It is seldom, however, that any of these conditions produce an appreciable diplopia, partly because there is in most cases at the same time a sufficiently accurate power of accommodation of the eye for different distances, but mainly owing to the intervening bridge which interrupts the rays of light being usually small, while the apertures which permit of their passage are not generally like mere pin holes, as in the experiment of Scheiner. It can be shown, as has been done by Schulek, that even when the eye is considerably out of focus only objects which are smaller than the intervening bridge give rise to double images, which accounts for the rarity of complaints of diplopia from patients in whom, from some cause or other, there is an opaque screen dividing the pupil into two parts. As a rule, the defect in vision produced in this way is not greater than to render the reading of very small type difficult or impossible. I have alluded to one other possible cause of monocular diplopia. This I have never seen, but it has been described by several writers,—first, I believe, by Javal,—viz., diplopia due to acquired faulty projection. It is said that in some cases of strabismus the individual, when using one eye alone, may be conscious of seeing simultaneously two images—the one corresponding to the macula lutea, and the other to that part of the retina which has been for long associated with the macula of the other eye.

Monocular diplopia which does not admit of an explanation in some way in accordance with the manners above described, though the exact cause may be difficult to detect, probably does not exist, although some cases have been ascribed to purely cerebral causes.

V.—NOTE ON A CASE OF RETENTION OF URINE FROM AN UNUSUAL CAUSE.

By J. HALLIDAY CROOM, M.D., F.R.C.P.E.

(Communicated to the Edinburgh Obstetrical Society, 9th January 1886.)

IN a paper which I read to this Society some time ago, I discussed in detail the various causes which gave rise to retention of urine in the female. As a short addendum to what I then said, and as, so far as I know, this cause is entirely unique, I propose to relate the following case:—

A young gentleman from one of the hotels in town brought, one afternoon, his wife to my consulting-room, telling me that she was in great distress from a swelling in her belly. Finding it impossible to examine her carefully in my room I went to her hotel, and found on examination a tense round dull tumour extending up to the umbilicus. She informed me that she had passed water recently, and had a constant desire to do so. Before proceeding further, I did what I always do in all pelvi-abdominal tumours, viz., introduced a catheter. As a result I removed about 2 quarts of urine. Being at a loss to know the cause, on inquiry I found that she had been married two days previously, and that since the night of her marriage she had complained of more or less distress, and had passed water only in very small quantities since. On examination, *per vaginam*, I found that the hymen, which was unusually thick and fleshy, and which was of the usual crescentic form, had been completely torn in the centre, and that the mucous membrane covering the posterior vaginal wall had been deeply lacerated for at least an inch. At the time of first intercourse there had been considerable pain, and some hæmorrhage, the patient stating that afterwards she had felt sick and faint. Both parties believing what had occurred to be the usual state of matters, the husband renewed his attempts later on in the morning, but since that time until she saw me no further intercourse had taken place.

I mention this case, firstly, for its extreme rarity. Irregular ruptures of the hymen have been already discussed in this Society, in an exhaustive paper communicated by Professor Schroeder, although no mention is there made of deep laceration of both hymen and vaginal wall such as that I have described. Twice before I have seen as a result of violent intercourse considerable and even deeper laceration of the posterior vaginal wall than in the present case, but unassociated with retention of urine. Secondly, the case belongs to that class of retention cases which are reflex in their causation. It belongs exactly to the same class as retention of urine in the puerperal woman from laceration

of the perineum, or as in retention arising from a urethral caruncle.

Winckel has shown, and in the paper by myself to which I have already referred, I found that catheterism is more frequently required in childbed, the greater the injury to the perineum, and it is probable that the relation between the necessity for catheterism and hymeneal laceration may be even closer.

The retention of urine is, in my opinion, not due, as some have said, to the patient voluntarily retaining her urine from the dread of allowing it to come in contact with a raw tender surface, and as a consequence, the retention becomes involuntary from over distension and temporary paralysis of the muscular coat of the bladder. Rather is it due to a reflex mechanism. It seems to me that this case, in common with all those of perineal laceration accompanied with retention, arises from tonic spasm of the sphincter vesicæ, caused by the stream of afferent stimuli reaching the centre from the nerve-endings in the lacerated wound.

VI.—ON THE TREATMENT OF LABOUR DELAYED BY OBSTRUCTION AT THE PELVIC BRIM.

By SAMUEL SLOAN, M.D., F.F.P.S.G., Obstetric Physician to, and Lecturer on Clinical Obstetrics at the Maternity Hospital, Glasgow; Vice-President of the Glasgow Obstetrical and Gynæcological Society.

(Read before the Society, 11th November 1885.)

(Continued from page 653.)

THE following are the quotations and cases referred to on pp. 635 and 641 of the January number of this Journal:—

Dr Goodell, the great American advocate for version, says,¹ "Cases there are in which turning cannot and should not be performed. Again, the urgency after that operation for immediate delivery is a strong argument against its indiscriminate use. Turning should generally be preferred to the lashing of the forceps handles. In pelves uniformly contracted, the forceps is the better means of delivery. In pelves narrowed in the conjugate diameter, turning should be resorted to whenever a half hour's faithful trial with the forceps fails to make the head engage. In pelves whose conjugates range from 2·75 to 3·25 inches, turning should be the initial step."

Dr Ramsbotham,² eight years after the first appearance of Simpson's papers on this subject, writes, "My own opinion is that, in the great majority of cases where craniotomy would be necessary, if the vertex presented, the same operation would be required after the shoulders were extracted; and that, therefore, the advantage to the child must be regarded as very limited. The pressure to which

¹ *Amer. Journal of Obstetrics*, 1875-76, p. 215.

² *The Principles and Practice of Obstetric Medicine and Surgery*, p. 222.

the funis umbilicalis must inevitably be exposed during its transit is peculiarly hazardous to the child. It is impossible to introduce the hand into the uterus and turn a child, even when the promise appears most favourable, without compromising the woman's security to some extent. In obstetric surgery there are some operations performed by the hand, equally dangerous as, or even more so than ordinary instrumental delivery."

"My own impression," says Dr Playfair,¹ "is that the use of the forceps will generally be found to be preferable to the original choice of turning. Where the head refuses to enter the brim, and cannot be sufficiently steadied by external pressure to admit of an easy application of the instrument, turning is simpler and safer. After turning moulding cannot be continued beyond five minutes without proving fatal. This, however, is no reason why turning should not be used after the forceps and the natural efforts have proved ineffectual."

"The measurement of the pelvis," remarks Dr Matthews Duncan,² "and especially the measurement of the conjugate, even if accurately made, is not the criterion of the mode of delivery to be adopted at the full time, or if premature labour is induced. In the same woman conditions may vary in different labours; and, in different cases of the same dimension, conditions may vary, so that at one time perforation may be the right operation, and at another time turning may be the right operation; and I may state to you that turning, or rather delivery by podalic extraction after turning, is not to be resorted to unless you have a rational prospect of getting a living child. If your delivery by turning ends in the delivery of a dead child it is, to a considerable extent, a failure; it would have been better to perforate—safer for the woman."

It is the opinion of Dr Leishman,³ that the forceps "in skilful hands is a safer operation to the mother. When the decision lies between turning and craniotomy," Dr Leishman says, "we must first be sure that, if we succeed in turning, the head can be got through the contraction; for it sometimes happens that, after turning, delivery can only be accomplished by perforating behind the ear. It must, therefore, be obvious that it would be better to perforate and deliver at once than to turn and then perforate, thereby subjecting the woman to a two-fold danger. The operation of turning, however," he adds, "when it can be effected, even after some time, and with some difficulty, is, there is good reason to believe, more safe to the life of the mother than that of craniotomy; so that, even when the child is dead, it is often to be preferred."

Dr Churchill⁴ expresses himself thus:—"Lastly, even if we succeed in selecting a suitable case and in extracting the child, it

¹ *The Science and Practice of Midwifery*, third edition, vol. ii. p. 79.

² *Clinical Lectures on the Diseases of Women*, p. 24.

³ *A System of Midwifery*, second edition, pp. 521, 522, 580.

⁴ *On the Theory and Practice of Midwifery*, fourth edition, p. 312.

has yet to be proved that the mother would not incur considerable danger from contusion or laceration in forcibly dragging the child through a narrow pelvis; for I must remind my readers that we have no statistics of the proposed operation to compare with those of the old method, the few cases adduced by Dr Simpson being of no value for this purpose. At the same time, I do not mean to deny that there are some cases in which it may be worth trying, as every successful case is a rescue of a life from destruction."

Dr Braxton Hicks,¹ an advocate for version, acknowledges that there is a risk of bruising and rupturing the uterus in attempting to save the child. If considerable doubt as to the result with the forceps, he would always prefer version; adding, "where there is a fair chance of the forceps succeeding I should always try it first."

Dr Schroeder,² "would distinctly caution the young practitioner not to rely upon the forceps when the head is high up in the contracted pelvis. We recommend," he says, "version in all cases of pelvic contraction where this is not absolute. Our reasons for doing so are—(1st), in the interest of the mother; (2nd), in very great pelvic contraction with a conjugate diameter of $7\frac{1}{4}$ centimeters (2·8 inches) we have extracted live children; and (3rd), because perforation of the aftercoming head is neither more difficult nor more dangerous to the mother than that of the presenting head."

Dr Spiegelberg³ insists that, "in contraction of the pelvis it will often be necessary to perforate; version must be reserved for certain cases, and the use of the forceps *rejected*."

The advice of Dr Caseaux is, "So long as any chance exists for the child version should be tried."

"At the present moment," says Dr Barnes,⁴ "it may be said that the chief advocates of turning are those who do not realize the advantage of a good long forceps. There are certain cases of deformed pelvis in which turning is certainly the best both for mother and child. The skill of the operator will in many cases determine his choice. If we fail after version, and have to perforate, we save the mother and have tried to save the child. There is no difficulty nor danger in perforating the aftercoming head. Assuming a standard head, the base of which unyielding measures three inches, this is obviously the limit beyond which the operation would be useless; for although the head is caught in the bitemporal diameter, a little in front of the biparietal diameter, the base must be exposed in its full width to the narrow strait. Even if the side of the head be indented by the promontory, no important degree of canting or obliquity can be counted upon. But if the

¹ *Guy's Hospital Reports*, 1870, p. 503.

² *A Manual of Midwifery*, pp. 280-282.

³ *Obstetrical Journal*, 1874-5, p. 392.

⁴ *Obstetric Medicine and Surgery*, p. 672.

head should be undersized or unusually plastic, there is a fair prospect of the child being drawn alive through a conjugate diameter measuring three inches."

Dr James Wilson¹ says, "Dr Simpson gives reasons for the preference of turning to craniotomy which appear to me quite incontrovertible."

Dr Lusk² declares that "so long as the head does not engage at the brim there is no rivalry between version and forceps. The latter should be placed under the ban as hardly less dangerous than the Cæsarean section." He, however, according to Dr Hodge,³ "concludes from his extensive review that the experience in Germany is decidedly in opposition to Simpson's views."

"It has been said," writes Dr Hodge,⁴ "that success justifies the means, but how a practitioner can be justified in a protracted case of delivery where the waters have been long evacuated and the body of the uterus firmly contracted upon the body of the child and placenta, and when a portion, if not the whole, of the presenting part has passed the circle of the os uteri, in attempting version, is inexplicable. No latent hope that the child might possibly be saved under these circumstances can compensate for the immense risk to the mother."

"The relative value," says Dr A. R. Simpson,⁵ "of two of the most important of these operations, viz., the operation of podalic version and extraction, and the operation of delivery with the long forceps, has never yet been finally determined." "In the great run of cases of flat pelvis where the condition is timeously recognised, delivery ought to be brought about by turning."⁶ Again, he states, that in the generally contracted pelvis, "the clear indication is for delivery by the forceps."⁷

CASE I.—Mrs M'L.—*first* confinement, March 1874; in labour four days; presentation "cross" (meaning breech), and the child, a boy of medium size, still-born. *Second* confinement, August 1875; labour two days and one night; delivered with the forceps after half-an-hour's "hard work" on the part of the accoucheur, who declared that he had worked with "all his might." The child—a very large boy—was still-born. *Third* confinement, September 1876; nineteen hours in labour; delivered by the forceps of a living girl, "much smaller than the boys." This girl lived for one year. *Fourth* confinement,⁸ 11th November 1878. My first attendance. Head occupied the brim. The anterior fontanelle was felt in front and to the left; the sagittal suture,

¹ *Glasgow Medical Journal*, 1856, p. 398.

² *The Science and Art of Midwifery*, p. 475.

³ *American Journal of Obstetrics*, 1875-6, p. 17.

⁴ *American Journal of Obstetrics*, 1875-6, p. 20.

⁵ *Contributions to Obstetrics and Gynecology*, p. 172.

⁶ *Ibid.* p. 170.

⁷ *Ibid.* p. 166.

⁸ *Glasgow Medical Journal*, 1879.

lying just behind the pubis, ran obliquely backwards and to the right, the part first touched by the finger on examination being *behind* this suture. The position of the head was thus peculiar, being, in fact, the third cranial position, but with the right parietal bone presenting instead of the left, on account of the obliquity of the head in the pelvis. Simpson's forceps was applied in the usual manner, and with no material difficulty, the blades lying one on either side of the pelvis. Little advance was made, though as much force was employed as I felt justified in using; and after the forceps had been fifty minutes on the head, it was still substantially where it had been at the time they were first applied. I was now driven to the conclusion that the forceps were preventing what nature was desiring; and if I had had a pair of straight forceps at hand, I should have expected a good result with them. Feeling persuaded, however, that if only a sufficient amount of *vis a tergo* were present, nature would be able to complete the delivery, or at least to rotate the head, I removed the forceps; and then placing my left hand on the fundus of the uterus, and passing my right arm under the right thigh of the patient, I clasped the fundus uteri in both hands, disposing the ulnar edge of each as far over on the posterior surface of the fundus as possible. Fortunately I secured a good hold; then fixing my breast against the side of the bed in such a position as to enable me to pull in the axis of the pelvis, I drew on the uterine contents during the next pain with all the force I was capable of exerting, when, to my great satisfaction, I found that I had brought the head quite to the outlet. Less force sufficed to complete the delivery, the head passing out in the second position. I now discovered the cause of the lateral flexion of the head, for the right hand of the foetus was found doubled up behind its right ear. The child—a very large boy—cried vigorously when born; and on examining the head for marks of the forceps, no injury could be found beyond a slight abrasion of the cuticle behind the left ear. The child did well; and with the exception of a mild attack of tonsillitis on the tenth day, the mother made an excellent recovery. *Fifth* confinement, May 1882. Forceps applied to head not through brim. Head descended with a jerk, which I felt and heard. There was a considerable indentation on the left frontal bone—the upper and inner portion—between the frontal eminence and the anterior fontanelle; but the edge of the bone forming the fontanelle was not depressed. The forceps had caught the head over the right frontal eminence and over the left ear. The head said to be broader than the boy's. The child, a girl, lived. The measurements of the pelvis were now taken, and were as follows:—Interspinous, 9 inches; intercrystal, $10\frac{3}{4}$ inches; diagonal conjugate, $4\frac{1}{4}$ inches.

CASE II.—Mrs S.—*first* confinement, seven months' child; no

assistance required; child born alive. *Second* confinement, twin miscarriage. *Third* confinement, midwife sent for Dr G. after many hours' severe labour. Head impacted at the brim. With Simpson's long forceps, after much trouble, delivered her of a dead male child of large size. Deep indentation over junction of frontal and parietal bones of one side. *Fourth* confinement, first cranial presentation; long forceps for delay at brim; head well ossified, and of moderate size; great force for over half an hour. Child born inanimate, but was resuscitated, and did very well, as did also the mother. Deep indentation (evidently a fracture) at junction of left frontal and parietal bones. *Fifth* confinement, delivered of a female child with long forceps; first cranial presentation; head indented on left frontal bone near junction with parietal; forehead slightly abraded from forceps. Mother and child did well. *Sixth* confinement, delivered by Dr G.'s assistant of a dead female child. Head marked as in the others. At the present date (November 1885) those indentations are perfectly visible. The children are healthy, strong, and intelligent.

CASE III.—Mrs M'D.—*first* labour, breech presentation; difficulty with the head; child still-born. *Second* labour severe; head, which had been the presenting part, was greatly elongated; child alive. *Third* labour, cranial presentation; head caught at brim; os half dilated; Simpson's forceps applied at 5.30 o'clock; lock quite within the vulva; handles wide apart, and lock tight and incomplete; traction brought down head slightly with a "click," but no further progress was made. Head found to be now in third position; forceps, which had been withdrawn, were again applied, and this time as nearly as possible in the left oblique diameter of the pelvis. The instrument now locked well, and the handles were much closer. The child was born about 6 o'clock, the head passing out in the second position after slight rotation of the forceps. Respiration was very difficult to establish, and the child could not be washed for about thirty minutes. It cried very much for two days, but eventually did well. There was a mark of the forceps on the middle of the back of the neck, and extending down to the level of the shoulders. There was also a mark on the upper part of the forehead, where there was a superficial cut of half an inch in length. The child was said to be the same size as the first.

These three labours were all attended by myself.

CASE IV.—Mrs P. had ten children in all. The first nine were born alive; the first two (large boys) with forceps; the sixth, the largest boy of all, said to have weighed 12 pounds, without forceps, and without difficulty in the establishing of respiration. The tenth child was a "plump" girl; the breech presented; the doctor had "a desperate fight;" the child was still-born. The same doctor had been with Mrs P. in all her confinements, and she told

me that he informed her he would have had no difficulty if the head had presented. Mrs P.'s health has been worse since this labour, and there has been considerable uterine trouble.

CASE V.—Mrs C.—*first* labour, forceps required; a large girl; head much moulded and marked by forceps; still-born. *Second* labour, forceps; small male head, marked and cut; child born alive. *Third* labour, a small male child; no aid whatever; child alive. *Fourth* labour, lingered for many hours, when midwife sent for Dr Ritchie, who delivered the patient by forceps of a dead child. There was a spoon-shaped depression over the vertex, near the anterior fontanelle, caused by the sacral projection. *Fifth* labour—the one about which I was asked to advise—was induced at eight months; child was born without assistance; it lived three days. *Sixth* labour, full period; cranial presentation; male child, alive; second stage did not occupy more than two hours; the child, which was of fairly average size (perhaps somewhat less in size than the fourth child), was expelled by *uterine effort alone*.

CASE VI.—Mrs J.—*first* labour, instrumental delivery; child still-born. *Second* labour, child born alive; delivered by forceps. *Third* labour, first cranial presentation; Simpson's long forceps; tight pull; male child, fair average size; child all right. *Fourth* labour, first cranial presentation; Simpson's long forceps; not so difficult as last; female child, smaller than the average; child all right. *Fifth* labour (5th May 1881), hand presentation easily converted into breech by introduction of hand, and bringing down both feet; no particular difficulty with body; delivery of head extremely difficult; tried at first manual traction; failed; attempted to apply forceps, but the fit was so tight that the blades could not be introduced alongside of head; then used much stronger manual traction, and after a severe pull, managed to complete delivery, the head having been born about ten minutes after the body; child inanimate, and remained so for about ten or fifteen minutes (heart beating all the time), when it gave a first inspiration. Child did well, though head swelled somewhat afterwards. It was probably less in size than the third or fourth. The children's heads were all extra well ossified, and in spite of strong and regular pains, remained above the brim. The conjugate diameter is lessened by at least half an inch.

CASE VII.—Mrs L. This woman was admitted into the Kennedy Street Temporary Hospital in 1879. She was at full time; the head presented; the late Dr Lyon's axis-traction forceps was applied, and after very great difficulty a large child was born, dead. The woman made a good recovery. She was advised to have labour induced should she again become pregnant. On 20th May 1881, she was admitted into the new Hospital for this purpose, being then, as was supposed, about $7\frac{1}{2}$ months advanced in

pregnancy. She is a rickety patient, and the true conjugate diameter is estimated to measure $2\frac{1}{2}$ inches. Barnes's bags were used, the largest one having been expanded so as to have a circumference of about 9 inches. The feet were found to be presenting. Twenty minutes after the rupture of the membranes, the body had entirely passed the brim, but not without a considerable amount of force being employed. The cord was found to pass between the child's thighs, and was three times round the neck. This having been undone, so far as possible, and the arms dislodged, traction was again employed in the axis of the inlet. Moderate pressure from above was also employed. Five minutes were thus spent in almost uninterrupted traction without much sign of advance on the part of the head. Free pendulum movement was then employed for one minute, when extraction was effected. Slow irregular contractions of the child's heart were observed for a short time after birth, but none of the usual methods of artificial respiration employed induced it to breathe. Child weighed 4 pounds 6 ounces. Biparietal diameter was $3\frac{1}{2}$ inches, but it could be compressed to 3 inches, the bitemporal was $3\frac{1}{2}$, and the bitemastoid $2\frac{1}{2}$. The circumferences of the child were taken, and found to be as follows:—

Ant. post. of head,	$12\frac{1}{4}$ inches.	Breech,	$10\frac{1}{2}$ inches.
Sub. occip. bregm.,	$11\frac{1}{4}$ "	Shoulders,	9 "
Pelvis,	$8\frac{1}{4}$ "		

There was a moderate amount of hæmorrhage from the cervix. Patient was weak for the first 30 hours, the pulse having risen, after the bleeding had had time to tell on it, to about 180. A vaginal injection of hot water stopped the hæmorrhage. The patient made an excellent recovery. Cervix found slightly split on right side. Considerable amount of swelling and tenderness over the upper part of the vagina, round the cervix, so that free examination could not be made.

CASE VIII.—Mary R., admitted to Maternity Hospital 5th December 1881, in pregnancy for the second time. The first labour the head had presented, and the child is said to have cried after having been "slapped." In the present labour, the feet presented, the upper part of the body was caught at the brim, the shoulders being out of reach of the finger. I passed my hand up gently, and drew down the shoulders, but this caused a very severe laceration of the perineum, which gave way like rotten cloth. The head was now retained in the pelvis, and had to be delivered by the forceps, which were in readiness. Seven minutes elapsed between the birth of the body and the extraction of the head. The child was with difficulty brought round. The mother said it was the same size as the first child born seven years ago. The weight of the present child was 8 pounds.

CASE IX.—“Mrs W. has had six children in all, four previous to that you were called in to complete the delivery of, and one since. Her *first* labour was a very severe one; four doctors in attendance and forceps used; child still-born; made a good recovery. In her *second* confinement the head presented; she was attended by a woman, had a tedious labour, which ended well with a living child of ordinary size. Her *third* and *fourth* children were delivered by forceps and were both still-born. Her *fifth* confinement was that in which you had a part. The presentation was *natural*, labour severe and tedious. Dr M., after trying the long forceps without avail, turned and brought down the feet. The head refused to follow, and I was then sent for in consultation. Attempts to extricate by traction were persisted in with melancholy result you know. The woman made a wonderfully favourable recovery! In her last (*sixth*) confinement, the head presenting, she was attended again by a woman, and was delivered of a living child. Her labour in this case came on a fortnight or so before she expected it.” There is no very marked deformity of pelvis, only a “slight projection of sacral promontory, narrowing the inlet somewhat antero-posteriorly.”

CASE X.—Mrs L., admitted into the Maternity Hospital 2nd May 1884. She has had two previous labours, which were difficult, the children having been delivered at the full time with forceps. Her last menstrual period was the latter end of August. Patient is small and rickety. The prominence of the sacrum is within easy reach of the finger, and the outlet roomy. The membranes are unruptured; the head lies above the brim but engaging, evidently caught on the sacral promontory. About 1 P.M. the head has been forced more deeply into the inlet, but was still caught on the promontory. About 1.30 the membranes ruptured spontaneously; the child was born naturally, the pains coming on forcibly. The child weighed $6\frac{1}{2}$ lbs., was a male, alive, and $8\frac{1}{2}$ months. The child's head is showing signs of regaining its natural shape. It was much lengthened and flattened on the vertex at birth, with a considerable depression of the right frontal bone. Patient was dismissed on the 12th, both doing well. About the end of August Mrs L. brought the child to show it; it was suffering from a recent attack of bronchitis, and looked a little pale, but was otherwise strong and healthy. The child is large and well nourished. There is still a marked depression in the right frontal bone, but the flattening of the vault has mostly disappeared.

CASE XI.—M. C., primipara, about 30 years of age and unmarried, was admitted 20th February 1881; was at full term; had crossed from Ireland to Glasgow with her avowed future husband, who had come to Scotland in search of employment. On landing she had been deserted by him, and had remained in a destitute

condition until she found refuge in our hospital. The impression of her face was vacant, and she presented symptoms very much akin to those of dementia in a mild form. The presentation was footling, and the cord, which was prolapsed, could not be replaced. As soon as dilatation was sufficiently far advanced to permit of podalic extraction, this was effected, but the head was caught at the brim. Traction with compression externally, after the manner recommended by Goodell, was successful after four minutes, and the child, a girl weighing $6\frac{1}{2}$ lbs., was born alive. A straight tear, half an inch in depth, was found to have taken place in the left side of the cervix. The death of the woman, which occurred on the eighth day, was due to septicæmia. This I feel sure was largely due to the mental shock she had received, and the destitution which preceded her admission to the hospital; but I think there can be little room for doubt that the lacerated cervix had a large share in the fatal result. At the *post-mortem* a tear was discovered in the right side of the cervix also. The pelvis in this case was transversely large, the interspinous and intercrystal measurements having been respectively 10 $\frac{1}{2}$ and 11 inches. Originally, therefore, that is before the flattening, a justo-major pelvis.

CASE XII.—Mrs M'G. This woman was admitted into the Maternity Hospital on 15th September 1880. Her first three children, girls, were born naturally; the next three, two boys and one girl, were delivered with forceps; the seventh, a boy, after failure with the forceps, was delivered by turning; and the eighth, a girl, was delivered by Dr Black with long forceps. All these children were born alive. The patient has made good recoveries from all her confinements except the seventh, her recovery from which was tedious. Patient was now in labour with her ninth child. Before admission to the Hospital the late Dr S. and Dr G. had tried the forceps, and, failing, had unsuccessfully attempted to turn. The woman was then brought into the Hospital, and was in a very weak state. It was deemed by those of the staff present, since there was a possibility of the child being alive, that we should try the forceps again; but that should they fail, perforation should be had recourse to. On the arrival, however, of Dr J. G. Wilson, who was consulting physician, and no mean obstetric authority, he recommended, seeing that the woman had rallied somewhat, that turning should be attempted if the forceps failed. This was done, and with great difficulty succeeded, the aftercoming head having to be extracted by the forceps. The child was still-born, and had the appearance of having been several days dead. The woman died, and from the state in which she came into the Hospital, I could not but feel that it would have been better to perforate at once than have made any attempt to save the child, which, though this was doubtful at the time, had been really dead. The pelvis was found to be only slightly contracted, but the child weighed $9\frac{1}{2}$ lbs.

CASE XIII. (reported by Dr Oliphant, house-surgeon).—Mrs G., admitted into Hospital 15th March 1884, in her third labour. Her first and second labours were forceps cases, and the children were still-born. In the present labour forceps had been tried, and, failing to deliver, turning was unsuccessfully attempted. The woman was then sent to the Maternity. When admitted her pulse was 150. She was very weak, and complained of great pain and tenderness over the upper part of the abdominal swelling. An examination of the abdomen gave the impression that the breech had escaped through a rupture of the uterus. The woman seemed to be sinking rapidly. Dr Sloan arrived, and decided to deliver her at once. Strong beef-tea was given, and a hypodermic injection of ether. Chloroform was administered, and perforation easily and rapidly performed. Barnes's long forceps and the blunt hook were applied, and the child extracted, the whole operation taking about ten minutes. The patient lived about one hour after delivery. At the post-mortem there was found to be a laceration of the vagina in Douglas's pouch. The conjugate diameter of the pelvis was 3 inches, and the transverse $4\frac{1}{2}$ inches. The child weighed fully $6\frac{1}{2}$ lbs.

CASE XIV.—This woman had nine children, forceps having been required for the first one only. There was a narrowing of the conjugate diameter. At this labour the forceps had been tried by three medical men, and turning was then attempted. This could be only partially carried out, and when I arrived I found the head at the brim and the two legs in the vagina. With considerable difficulty I pushed up the legs sufficiently to reach the head, and perforated it; but found that even then it was impossible, on account of the legs being in the way, to extract the head. Turning was again attempted as the only way to deliver, and this was now an easy matter. The woman died, as was expected.

CASE XV.—The following case has come into the Hospital since this paper was read, and, as it is an illustration of what I am desirous of emphasizing, I give a brief report of the case here, supplied to me by my house-surgeon, Dr John Brownlie. Patient admitted 13th Nov. 1885, at 5.20 A.M.; unmarried, secundipara. Her first confinement took place about eleven years ago; the labour was normal; presentation cranial; child full time, alive, and of good size. The membranes at this, the second labour, had ruptured five days before admission into the Hospital, and pains came on, but passed off again. They returned on the day of admission. On coming into the Hospital, the right hand was found presenting low down at the vulva, and the head was lying in the left iliac fossa. The uterus was quiescent. The os was dilated to about the size of a crown piece. Chloroform was administered, and Dr Sloan performed podalic version, bringing down the right foot. Labour was then allowed to proceed. Dr

Sloan, finding that the margins of the os were thin and hard, gave special injunctions that the head was not to be drawn rapidly through the cervix. Soon after patient recovered consciousness, the uterus began to contract, and the leg and breech descended. The left leg remained extended over the abdomen of the child. The body was drawn forward, and the leg released. The arms and shoulders were then drawn down, some difficulty being experienced in the case of the right arm. As the cord had by this time become pulseless, the head was delivered as speedily as possible. It did not, however, come easily, and the difficulty appeared to be at the cervix. Pressure on the occiput, the child being drawn forward over the abdomen of mother, finally brought the head down. It then passed through the cavity with great ease. About five minutes were occupied in the passage of the head. Although the heart was beating, artificial respiration failed to restore animation to the child, which was a small one. The interspinous measurement is 9 $\frac{1}{2}$, the intercrystal 10 $\frac{1}{2}$. Patient made an uninterrupted recovery, the only rise of temperature of any consequence being on the day of her admission. She was so well as to be discharged (at her own request) on the eighth day. On vaginal examination it was found that there was absolutely no irregularity round the os. In the posterior wall of the cervical cavity there is a little elevation of surface resembling a small tubercle. There is no cicatrix, nor any appearance of a tear.

Part Second.

REVIEWS.

A System of Practical Medicine by American Authors. Edited by WILLIAM PEPPER, M.D., LL.D., Provost and Professor of the Theory and Practice of Medicine and of Clinical Medicine in the University of Pennsylvania; assisted by LOUIS STARR, M.D., Clinical Professor of Diseases of Children in the Hospital of the University of Pennsylvania. Vol. I. Pathology and General Diseases. Vol. II. General Diseases (continued) and Diseases of the Digestive System. Vol. III. Diseases of the Respiratory, Circulatory, and Hæmatopoietic Systems. Philadelphia: Lea Brothers & Co. London: Sampson Low & Co.: 1885.

It is no barren outcome of conventional politeness to say that this magnificent work has filled us with feelings of warm admiration. We entirely agree with the editor in thinking that the time has arrived for presenting the whole field of medicine as taught and practised in America, and we heartily welcome this System of

Practical Medicine, which gives such a complete picture of the present state of medicine among our kinsmen across the Atlantic. The work is adorned by a galaxy of famous names, many of them familiar to the European student as representative of the best work done in scientific medicine on the Western continent, and the articles are therefore to be regarded as coming from the highest authorities on the particular subjects of which they treat.

It is naturally out of the question to give, on an occasion like this, an adequate criticism of the contents of such a work as the *System of Practical Medicine* now before us. All that we can do is to state our impressions of some of the articles that we have been able to peruse, and before doing so we would remark that our silence regarding other and equally able essays is not caused by any invidious comparisons, but solely by want of space.

The first volume begins with an excellent article on General Morbid Processes, from the pen of the Professor of Pathological Anatomy in Harvard University. It is characterized by a judicial tone, highly necessary in times of such rapid advance as our own. The most recent discoveries are embodied in this contribution, as, for instance, in the section of it devoted to Tuberculosis, but they do not usurp an undue share of it, as they often do in our modern works, for the author finds space, not only to present an adequate picture of the present state of our knowledge, but to give in addition a very full and clear historical sketch of the views held regarding tubercle.

Hartshorne writes the article on General Etiology, Medical Diagnosis, and Prognosis, and it contains a vast amount of most useful information. His observations on heredity or pre-natal causation are excellent, and we notice that under conditional or atmospheric causation he has not omitted to mention the interesting observations on the low barometric readings which precede disease, and the now well-known fact that the results of surgical operations are most satisfactory when the barometer is high or on the ascent. *Enthetic* causes of disease are treated from a scientific point of view, and a careful classification of all known morbid organisms is given, which is accompanied by beautiful illustrations. We have been particularly struck by the portion of this article dealing with medical diagnosis, where, under the title of Symptomatology, a great many valuable facts have been carefully gathered together.

The section on Hygiene is by Billings, and we need only say that it is worthy of its author, who divides his subject into—1. Causes of disease, means of discovery, and prevention; 2. Personal hygiene in its relations to the practice of medicine; and 3. Public hygiene in its relations to physicians. The subject is treated altogether in that philosophic manner which we are accustomed to find in the writings of Billings.

The article on General Pathology and Sanitary Science deals with drainage and sewerage in their hygienic relations.

This has very wisely been entrusted to a sanitary engineer, who has done his work well. While studiously refraining from any invasion of the province of the physician, Waring lays down excellent lines for practical sanitation, and no one can read the paper without learning something from it.

General Diseases from Special Morbid Agents operating from without occupy the rest of the volume; and Simple Continued Fever, Typhoid Fever, and Typhus Fever are contributed by Hutchinson, of Philadelphia. It is to us a matter of great satisfaction to find that he points out that the temperature in enteric fever is rarely typical. The majority of teachers during the last two decades have pinned their faith to Wunderlich's classical temperature chart, with baneful results in the practice of their students during their early years; and we are much pleased to find Hutchinson writing as follows:—"The febrile movement, however, rarely follows a perfectly typical course, and I consequently find, in looking over the temperature sheets of a large number of cases, very few which bear, except during the first period, anything more than a general resemblance to the chart which Wunderlich has prepared as typical." One other point we should like to mention with gratification. The author is not enamoured of the cold bath as a means of treatment except in selected cases of hyperpyrexia.

Relapsing Fever is written by the editor. He allows that scarcity of food may be one of the most powerful causes of the disease, but gives his own experience in favour of overcrowding and filth as the commonest factors in its production. There can be no doubt that both relapsing and typhus fever have been very greatly reduced, in frequency at least, in Scotland since the passing of the Poor-Law Act; but there have been other causes at work in this amelioration, such as the opening up of densely populated districts and the removal of insanitary houses. It is well to remember, also, the observations of Muirhead, which are quoted in this volume, on the breathing-space of each individual in the last Edinburgh epidemic. Pepper discusses the Etiology very fully, entering into the question of contagion and the discovery of the spirillum without bias or prejudice. The article is of very great value, as being based on close personal observation of numerous cases of the disease.

The other general diseases are discussed by Hyde, Foster, Louis Smith, Hardaway, Bemiss, Keating, Jacobi, Stillé, Wilson, White, Schmidt, Watson, and Lusk. The papers contributed by these writers appear to be of very great value. They are all brought down to the present date, and may be regarded as authoritative monographs on their respective subjects.

Before leaving this volume we should like to add that the editor deserves high praise for getting the article on Beriberi, from the pen of Simmons, of Yokohama, who is well known as the author of a recent report on this disease, and for handing over Rabies, Hydro-

phobia, Glanders, and Anthrax to the Professor of Veterinary Science in Cornell University. These two contributors have very considerably enhanced the value of the work.

The second volume contains General Diseases from Derangements of the Normal Processes of Nutrition, and Diseases of the Digestive System, and it quite maintains the high standard with which the System set out in the first volume. The article on Rheumatism, written by the Professor of Medicine in the M'Gill University, is a masterly summary of our present knowledge regarding all the forms of that disease. Not only does it contain full references, with critical remarks thereon, to all the most recent additions to our knowledge of rheumatism, but its importance is much increased by the author's own personal observations.

The article on Gout, by Draper, is also exceedingly good, and the author does not allow himself to be led away by the many ingenious and beautiful theories lately advanced from a thoroughly practical aim. Rachitis has been placed in the hands of the proper author, and Jacobi treats the subject in a way worthy of his well-earned fame. Scurvy and Purpura are well-written essays, bringing their subjects well down to the present time—that on Purpura including the recent views brought before the Liverpool meeting of the British Medical Association, and afterwards expanded elsewhere.

The article on Diabetes Mellitus has been confided to Tyson, and it is not too much to say that he has performed his task in a way that will increase his already great reputation. He enters fully into the physiological and pathological aspect of the disease, and illustrates his meaning by capital diagrams; but he is, of course, compelled to admit that the etiology and pathogenesis of the disease are both as yet in an unsatisfactory state. In connexion with the question of diabetic coma, which he fully discusses, it is of interest to note that the author has in his hands "a woman, who considers herself in perfect health, but in whom there remains a trifling glycosuria, who at one time was supposed to be dying of diabetic coma." The tests for sugar are, as might be expected, carefully described and impartially criticised. The dietetic, hygienic, and medicinal treatment receive full justice, and we are glad to observe that Tyson has modified the opinion which he formerly held as to the greater usefulness of ergot as compared with codein. He now prefers the latter, but is uncertain as to its *modus operandi*, and we cannot wonder at this, seeing that it is difficult to make clinical observation fit in with the investigations of W. von Schröder on the opium alkaloids.

The articles on Scrofula and Hereditary Syphilis are very good; and the remainder of the volume, devoted to Diseases of the Digestive System, is of high and uniform excellence. We have not time to enter upon a detailed criticism of the various essays contained in it, but would like to remark in passing that Bartholow's

treatise on Diseases of the Liver is a model of careful and learned writing, while Alonzo Clark's contribution on Peritonitis is a most classical and scholarly essay.

The third volume contains the Diseases of the Respiratory, Circulatory, and Hæmatopoietic Systems. Laryngoscopy and Rhinoscopy are ably discussed by Seiler; and the articles on the various affections of the Larynx, Trachea, and Bronchi are excellently written by Allen, Johnson, Jacobi, Elsberg, Lefferto, Davis, Geddings, and Chew. The latter writer also contributes the sections on Emphysema, Atelectasis, Congestion and Œdema of the Lungs. Carson's essays on Hæmoptysis, Pulmonary Apoplexy, Abscess and Gangrene of the Lung are at once erudite and practical, and the historical introductions to all of them are written in a classical vein rare in these days. As might be expected, Croupous Pneumonia could not receive better treatment than it does at the hands of Loomis; and we may say the same of Catarrhal Pneumonia, which is contributed by the editor. Beverley Robinson gives an excellent account of Pulmonary Embolism; and when we say that Pulmonary Phthisis is from the pen of that great master, Austin Flint, we say enough to assure our readers of its great value. Two-fifths of the article are devoted to treatment, including climatic, dietetic, and regimenal and medicinal agencies, and the author dwells on the introduction by Pepper of injecting medicated fluids into vomicæ. A most important part of this subject is the "Prevention of Phthisis," in which Flint wisely observes:—"The prophylaxis against phthisis must date from birth." Syphilitic Disease of the Lung, Pneumonokoniosis, Cancer of the Lungs, and Pulmonary Hydatids receive adequate treatment at the hands of Bruen. Lynch contributes the essay on Acute Miliary Tuberculosis. It is a most excellent production, and we are glad to observe that he takes a hopeful view of it, expressing his opinion that he has successfully treated many cases of primary pulmonary tuberculosis. The Diseases of the Pleura find an able exponent in Frank Donaldson.

The Diseases of the Circulatory System occupy about a third of the volume. The Diseases of the Substance of the Heart form the subject-matter of the first essay, written by Osler, who has recently left Canada for Philadelphia. This article is a very able one, our only fault with it being that it has too little of the author's own individuality. We might advance the same objection to the essay which follows on Endocarditis and Cardiac Valvular Diseases, by Loomis. It is, if possible, too judicial and unbiassed in its tone, but, like the article by Osler, it is of great worth. The article on Cyanosis and Congenital Anomalies of the Heart and great Vessels, by Longstreth, and that on Cardiac Thrombosis by Beverley Robinson, deserve notice as being characterized by intimate acquaintance with their respective subjects in every way; and our only feeling regarding Austin Flint's essay on Neuroses of the Heart is one of regret that it is so short. In the able article on

Diseases of the Pericardium, by Da Costa, we find that he believes in the possibility of idiopathic pericarditis, but he does not state that he has ever met with a case. A noteworthy feature of this volume is the contribution by Roberts on the Operative Treatment of Pericardial Effusions, a subject on which he has established a right to be heard. The diseases of the aorta, of the arteries, and of the veins receive adequate recognition, and are followed by an interesting account of the Caisson Disease by A. H. Smith, who carefully studied the affection during the construction of the Brooklyn Bridge. The diseases of the mediastinum complete the thoracic affections.

The Diseases of the Blood and Blood-Glandular System have been entrusted to Osler, who is able to speak with authority on such a subject, and who in this essay is at his best. After an introduction, he treats of plethora, anæmia, chlorosis, melanæmia, progressive pernicious anæmia, leukæmia, Hodgkin's disease, hæmophilia, Addison's disease, and other diseases of the suprarenal bodies. It is one of the best contributions to the work, and we have glanced over it with great pleasure. The Diseases of the Spleen are described by Atkinson in a clear and succinct manner; the Diseases of the Thyroid Body are shortly discussed by Hayes Agnew, and the third volume ends with a brief account of Simple Lymphangitis by Busey.

It goes without saying that the printing, paper, and illustrations are alike excellent. Why the third volume, however, should be in smaller type than the first two is a mystery to us, and some curious misprints in the matter of proper names, such as Guttman, Bigbie (as they are given in the work), and others, seem inexcusable. The indices to the work are very full and very correct.

There is very little overlapping on the part of the authors, and therefore but little repetition. If we were to venture on any adverse criticism of the work as a whole, we should say that its various writers show too little individuality; but this is to be preferred to violent partisanship. The tone throughout is markedly scientific, even judicial, and the authors have taken care rather to give us the present aspect of medicine than to intrude their own views.

A decade has nearly passed away since our kinsmen in America fittingly celebrated the hundredth anniversary of the birth of the nation, and each year of that period has seen a more hearty mutual appreciation on the part of the sister countries. Perhaps this has been seen more in science and medicine than in any other walks of life, and the scientific workers on both sides of the Atlantic have gained thereby. We would offer our congratulations to the editor on the excellence of the *System of Medicine*; and in expressing our high opinion of the work, we have only to add our hearty wish that it may be read as much in this country as it deserves.

Clinical Lectures on Diseases of the Liver, Jaundice, and Abdominal Dropsy, including the Croonian Lectures on Functional Derangements of the Liver, delivered at the Royal College of Physicians in 1874. By CHARLES MURCHISON, M.D., LL.D., F.R.S. Third Edition. Edited by T. LAUDER BRUNTON, M.D., Sc.D., F.R.C.P., F.R.S., Assistant Physician to St Bartholomew's Hospital, London. The Section on Tropical Diseases by Sir JOSEPH FAYRER, K.C.S.I., LL.D., M.D., F.R.C.P., F.R.S. London: Longmans, Green, & Co.: 1885.

It is with melancholy feelings that we take up this new edition of a familiar work, for its perusal recalls the sense of personal loss experienced on the decease of its lamented author. At this time it would be out of place to express any opinion of a work which has for so long been highly esteemed by the medical world as the most useful guide to the subjects of which it treats, and in the few remarks which follow we shall confine our attention to some of the changes which we find in this new edition. The editor has approached his task with the feeling that he would best perform his duty by trying as far as possible to preserve the individuality of the work, and he has therefore placed his alterations and additions in brackets, so that they can at once be distinguished by the reader. With a few exceptions the changes are insignificant, simply bringing the work up to the present state of our knowledge, and they are all introduced with the modesty and taste which are so characteristic of the editor. The section on Tropical Abscess has been considerably enlarged by Fayrer, who has added an interesting statement regarding the Relation of Liver Abscess to Dysentery, a useful series of statistics bearing on hepatic abscess, and several additional cases, while he has enlarged the part of it dealing with treatment, especially with respect to operative interference. His description of the method of operating adds much to the value of the work for intending Indian medical officers.

The mode of action of toluyendiamine in causing jaundice and hæmoglobinuria is clearly and succinctly explained by Brunton, who gives the results of Stadelmann's experiments, and thus adds a most interesting section to the work.

Jaundice of nervous origin, which was only dealt with somewhat briefly by the author, is supplemented by some interesting statements by Brunton, who brings forward the facts of experiments on the sympathetic and pneumogastric nerves in the increase and diminution of bile as a confirmation of Murchison's teachings. Brunton further points out that there is another interpretation of true *icterus neonatorum* than that given by the author, and he prefers to regard the whole subject of the destruction or transformation of the biliary constituents as still obscure.

The section on the Surgical Treatment of Gall-stones is a most

useful addition to the work, and the subject of cholecystotomy is fully discussed, the valuable labours of Marion Sims, Lawson Tait, Musser, and Keen being brought forward. As this is one of the many points in which surgery has come to the aid of medicine in recent years, it is a very valuable addition to the work.

Another most interesting addition is that containing Brunton's remarks on the important function of the liver in preventing, not only bile, but other substances, from entering into the general circulation, a subject which he illustrates with a great wealth of observations.

In addition to these and some other similar additions to the work, Brunton has, as might be expected, made considerable additions in the matter of remedies, more especially in the concluding sections on Functional Derangements.

All the alterations have been made in a reverent and tender spirit, which will be appreciated by the reader. The editor and Sir Joseph Fayrer are to be congratulated on having at once done so much and so little, so that, while thoroughly representing the state of our present knowledge, the work remains as valuable for the present time as it was for the period when it left the hands of the author.

Clinical Lectures on the Practice of Medicine. By the late ROBERT J. GRAVES, M.D., F.R.S., Professor of the Institutes of Medicine in the School of Physic in Ireland. To which is prefixed a Criticism by Professor TROUSSEAU. In two volumes. Vol. II. London: The New Sydenham Society, 1884.

WE can only repeat what we said when the first volume of the Clinical Lectures appeared as one of the New Sydenham Society's series, that we think the Society has been right in reprinting Graves' works, and in saying this we desire to express the thanks of the profession to them for doing so.

Contributions to Pathology and the Practice of Medicine. By JOHN RICHARD WARDELL, M.D. Edin., Fellow of the Royal College of Physicians; Consulting Physician to the General Hospital, Tunbridge Wells. London: H. K. Lewis: 1885.

THIS bulky volume had not long made its appearance when its author finished his life's work. It consists of a series of fifty essays, the result of reading and of observation, and it need hardly be said that the most interesting and valuable part is the record of the author's own experience. The book will remain the monument of his industry, while it testifies to the love he bore his profession, and the ability with which he practised it.

Practical Histology and Pathology. By HENEAGE GIBBES, M.D.
Third Edition. London: H. K. Lewis.

WE welcome a third edition of this little volume, in which the author, who thoroughly understands his subject, deals with it, as before, in a masterly manner, and gives his readers the benefit of his ever-growing experience in this form of practical work. Those methods only are advanced, and we may particularly refer to "multiple staining," which have been by himself, and from often repeated trials, found deserving of record. The whole book has undergone complete revision; every part of it is well worthy of careful study, and but few workers in this direction can, in our opinion, well afford to be without it.

School Hygiene and the Diseases incidental to School Life. By ROBERT FARQUHARSON, M.P., M.D., LL.D., etc. London: Smith, Elder, & Co: 1885.

DR FARQUHARSON'S work appears opportunely after the recent discussion on over-pressure in schools. It is a pleasant and very readable volume of some 360 pp., printed, as all educational works ought to be, in excellently clear type. The subject of School Hygiene is treated under six heads,—Buildings, Diet, Work, Play, Duties of the School Doctor, and School Diseases. The work throughout, but especially the first chapter of it, appears to be written with particular reference to such public schools as Rugby, Marlborough, Fettes, etc., but governors of smaller institutions, members of school boards, schoolmasters and schoolmistresses, will find in it information that will be of much value to them. In discussing the arrangement of buildings for large public and private schools, the author gives his preference to the separate grouping of masters' houses round the large central schoolhouse, instead of the hostel system which assembles all the boys under one roof. This has not only the advantage that the boys under each master can be treated more as members of one family, and thus have more individual supervision, but that if an epidemic should unfortunately break out, isolation can be more readily effected than if they are all crowded together under one building. The hygiene of the dormitories and the school-room itself is enlarged upon, and the parts which badly arranged light and ill constructed seats play in the causation of myopia, spinal curvature, and other affections, pointed out. The chapter on School Diet gives an opportunity for the exposure of those curious fads of trainers which boys think it necessary to subject themselves to when preparing for some athletic contest. Under School Work special reference is made to Sir James Crichton Browne's Report

on Educational Over-pressure. Dr Farquharson, while agreeing with much that Sir James has said, is evidently of opinion that it is only in unhealthy and underfed children that such evil results as were depicted are seen. The chapter on School Diseases is one of the best in the book so far as it goes. It contains special contributions by Dr Broadbent, Mr Edmund Owen, and Mr Brudenell Carter. It may be well to point out that the Nelson's apparatus, so highly recommended by Dr Dukes, has not proved so satisfactory in other quarters as his enthusiastic testimonial would indicate. The absence of an index is a blemish that we shall hope to see remedied in a future edition.

Insanity: Modern Views as to its Nature and Treatment; a Portion of the Morison Lectures on Insanity delivered in 1879. By W. T. GAIRDNER, M.D., LL.D., Professor of Medicine in the University of Glasgow. Glasgow: MacLehose: 1885.

THIS pamphlet is noteworthy as being the last of the Morison Lectures on Insanity in which the lecturer actually did treat of insanity, instead of some other subject with which he was better acquainted. In addition to the address, which only occupies eighteen pages, we have an appendix of notes which fill about thirty more. The address is in every way worthy of its distinguished author, and we have found the notes interesting reading. The first is "On the Early Training and Mental History of John Stuart Mill, considered in reference to some peculiarities in his Writings." This is followed by a critique on "The Subjection of Women." After a very able fashion Dr Gairdner shows the false and superficial character of this work, whose evil effects have not yet passed away.

The third note is on "Witchcraft and Demoniacal Possession," in the course of which Dr Gairdner gives, from his own observation, a description of "a modern witch medically investigated," i.e., a woman affected with symptoms which would most likely have caused her to be burned for a witch had she lived in our country in the beginning of the 17th century. She was an ungainly looking creature, with a wry neck and a habitual tremor. Her ways were unsociable, and she described herself as not being able to feel anything as she ought to do, which made her tired of life.

La Morphinomanie. By B. BALL. Pp. 166. Paris, 1885.

THE principal paper in this little book treats of Morphinomania. Dr Ball tells us that this vice is to-day so much diffused in Western Europe and in France, that it has seemed to him useful to republish the lectures which he had already delivered in the Clinique

of the Medical School of Paris, in a form accessible "au grand public." Any medical practitioner who has been called in as adviser in one of these most distressing and discouraging cases, will obtain a clear and comprehensive view of the nature of the malady, and the resources of the healing art, by reading Dr Ball's little treatise. Morphinomania, he tells us, exists when the individual feels an irresistible desire to take morphia; and as dipsomania leads to alcoholism, so morphinomania inevitably leads to morphinism, that is, the morbid condition resulting from the continued use of opium or its compounds. Dipsomania is an intermittent neurosis. The craving for drink does not always exist in the mind of the dipsomaniac, and his disease is absolutely incurable, like most of the neuroses in which the crisis returns by fits. Morphinomania, on the contrary, is a continuous neurosis. Its victim feels a perpetual craving for his stimulant, but it is a curable malady which has often been cured. Thus drunkards who get intoxicated every day may be cured of their morbid cravings; dipsomaniacs, never.

Dr Ball recalls the famous soliloquy of Hamlet, where the prince asks, Were it not for the fear of something after death, who would bear the evils of life when he could make his quietus with a bare bodkin? This bare bodkin of which Shakspeare speaks, which has to free us from our misery, is the subcutaneous injection syringe. With one puncture we can assuage the sufferings of the body and of the mind, feel beyond the injustice of men and the slights of fortune, and pass into the pleasant dreamland of opium. This explains the irresistible attractions of the drug.

In three lectures, filling 70 pages, Dr Ball gives us a complete account of the characteristics of morphinomania, the effects of abstinence from morphia, and the diagnosis, prognosis, and treatment.

The fourth lecture is on "The Frontiers of Insanity;" the fifth on "Cerebral Dualism," the curious subject which was treated of by Dr Wigan in his book on the Duality of the Mind. The remaining lectures are "On Prolonged Dreams," and on "Insanity Occurring in Twins." While the lectures on morphinomania form a serious medical treatise, the other essays are written in a style which ought to interest intelligent readers of all classes. The cases cited by Dr Ball are well chosen, and are related with amusing point and vivacity. To no living writer is morbid psychology more indebted than to the Professor of Mental Diseases in the University of Paris, who is also the editor of the *Encéphale*. While we grant that the French School is second to none in the study of insanity, we fancy that Britain can claim some reflected credit in our author, to judge from his name and the perfect manner in which he was heard to use our language at the International Medical Congress in London, and the Centenary meeting at Edinburgh. At any rate it may be justly said that Dr Ball unites French epigrammatic force with English thoroughness and cosmopolitan learning.

Materia Medica, Physiological and Applied. Vol. I. London: Trübner & Co.: 1884.

THIS is a book of 726 pages, and is only the *first* volume of the *Materia Medica*. There is no indication given of the number of volumes to follow; but if the *Materia Medica* be at all comprehensive, and the succeeding volumes as large, it will be indeed a most ponderous work. This volume only treats of six substances: two inorganic, *Kali bichromicum* and *Plumbum*; three vegetable substances, *Aconitum*, *Digitalis*, and *Nux vomica*; and one animal substance, *Crotalus*, the poison of the rattlesnake.

The treatise on *Aconitum* is by R. E. Dudgeon, M.D.; on *Digitalis*, *Nux vomica*, and *Plumbum*, by the late F. Black, M.D.; on *Kali bichromicum*, by J. J. Drysdale, M.D.; and on *Crotalus*, by J. W. Hayward, M.D. The preface is signed by Drs Drysdale, Dudgeon, Hayward, and Hughes. In a somewhat lengthy preface they discuss the general action of medicines from their own standpoint as homœopathists. In their first sentence they state as an axiom, that "all drugs must act therapeutically only by virtue of their physiological action on the healthy body,"—a statement which has never been proved, and one which is known not to be correct. The action of a drug in health and disease is not necessarily the same. It would serve no purpose here to enter on a review of the peculiar tenets of homœopathists. Suffice it to say, the present volume shows great research on the part of its authors; but they lack sadly the power of digesting their material and putting it within proper limits. When we remember that the present volume of 726 pages only treats of *six* substances, it must be self-evident that few will ever read through, far less digest, all that is likely to be written.

If any one will take the trouble to read the article on *Crotalus* by Dr Hayward, he will be astonished to find how many diseases may be cured by it; in fact, it seems a kind of panacea for all the ills to which flesh is heir. It is recommended for delirium tremens, mania, paraphasia, general paralysis of the insane, vertigo, vomiting, convulsions, headache, meningitis, apoplexy, ophthalmia, hæmorrhages, glaucoma, deafness, frostbite, erysipelas, acne, mumps, cancrum oris, cancer of tongue, diphtheria, gangrene, cancer, dyspepsia, peritonitis, jaundice, bubo, diarrhœa, dysentery, cholera, after-pains, suppression of urine, "affections of the urinary and sexual organs," asthma, cough, varicose veins, palpitation, carbuncles, boils, compound fractures, cramps, gonorrhœal rheumatism, chilblains, psoriasis, ulcers, syphilis, whitlow, lupus, burns and scalds, "disorders of sleep," fevers, variola, scarlatina, glanders, chorea, dropsy, etc., etc. These are some, and only some, of the many diseases for which *Crotalus* is recommended. A treatise which can recommend one single medicine for such a variety of diseases is not likely to become a general favourite with the profession.

Burr's Medical Index. Manufactured by the BURR INDEX COMPANY, Hartford, Conn.

MESSRS MACLACHLAN & STEWART, the Edinburgh agents, have sent us a copy of this Index, which is specially adapted to the use of physicians and surgeons who desire to preserve memoranda of their reading, or of observations made in the course of practice. Most medical men who seek to keep themselves abreast of the daily increasing professional knowledge find a common-place book of some kind a real necessity. The book before us supplies this need better than anything of the kind we have seen. It is a strongly half-bound quarto volume, the pages of which are divided into two columns—a smaller to the left hand side for the name or subject, and a larger for the note or reference. The pages are indexed by means of projecting tabs stamped on both sides with the letters of the alphabet, and further by aid of thumb-hole cuts in which are printed, also in alphabetical order, the first two letters of the word to be entered in the name or subject column. Thus, to enter or to find a note regarding Pneumonia, the reader would throw the book open at the page from which the tab stamped P projects. In one of the thumb-holes before him he would then find the letters PN, which guide him at once to the page on which to enter or find the desired note, the word "Pneumonia" being placed under the initial letters PN (which are printed in the small column as well as in the thumb-hole), and the note being opposite it in the larger column. The spacing has been, we are told, a piece of arduous work, "requiring long, specific, and accurate study and calculation—no idle guess work." For example, the scrutiny of a number of physicians' note-books has shown that there is required "five times as much spacing or blank leaves under the subject-initial letters, Ab, Ca, In, Ov, Sy, Ut, as under some other letters." Such a book will, we doubt not, prove a very great convenience, and ought to become popular.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH, SESSION LXV.—MEETING III.

Wednesday, 2nd December 1885.—Prof. GRAINGER STEWART, *President, in the Chair.*

I. ELECTION OF ORDINARY MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—Robert W. Felkin, L.R.C.P. & S. Ed., F.R.S.E.; S. Hall

Puckle, B.A. (Cantab.), M.B., C.M. Edin.; James Haig Ferguson, M.B., C.M. Edin., M.R.C.S.

II. EXHIBITION OF PATIENT.

Mr Bennet showed a female child, 6 weeks old, suffering from CHRONIC INDURATION OR CONGENITAL TUMOUR OF THE LEFT STERNO-MASTOID. The condition was first noticed by the mother a week after birth. The whole of the muscle except the attachments was affected, there being in its place a dense indurated tumour or rigid band, over which the skin was freely movable. There was permanent wry-neck. This affection had been ascribed to various causes, such as to simple hypertrophy, to injuries received during birth, and to syphilis, but that had been abundantly disproved. Dr Frederick Taylor had shown by post-mortem examination that there was a development of fibrous tissue in the muscle, which ultimately destroyed the muscular fibres. In most cases the *right* side was the one affected, hence the theory of its occurrence in certain foetal presentations. In this instance it was the *left* side, and that more extensively than was usually described. The labour had been normal, and the mother had already given birth to nine or ten healthy children.

Mr Duncan had seen a similar case that morning at hospital, but the affection was on the right side.

Mr Joseph Bell had seen the condition four or five times in all. His experience was that it disappeared in two or three months.

III. EXHIBITION OF PATHOLOGICAL SPECIMEN.

Mr Skene Keith showed (1) A FIBRO-CYSTIC TUMOUR of the UTERUS, which, with its fluid contents, weighed almost 15 pounds. It was composed of one large cyst, and at no point was the wall an inch in thickness. The growth of the tumour had been very rapid, and it was first noticed fully two months after the cessation of menstruation at the age of 44. In one week it grew from the level of the umbilicus up to the sternum, and slightly raised the ribs. (2) A VERY MUCH LOBULATED UTERINE FIBROID, which weighed $11\frac{1}{2}$ pounds. The bladder had been pushed out of the pelvis by the tumour, and extended upwards to within 2 inches of the umbilicus. Fortunately, however, it was not adherent, and slid back into the pelvis when the tumour was drawn out.

IV. EXHIBITION OF INSTRUMENT.

Dr Foulis showed an IMPROVED CATHETER to be used with the instrument he had shown at a previous meeting for washing out the bladder. It consisted in a stopcock fitted at the end of the catheter, which, when closed, prevented any of the fluid passing to the bladder escaping, or of air entering. The same thing had to be done before by the finger or thumb of the surgeon, but that rendered the passage of the catheter more difficult.

V. ORIGINAL COMMUNICATIONS.

1. *Mr Annandale* read his paper on THE OPERATIVE TREATMENT OF POPLITEAL ANEURISM, which appears at page 715 of this Journal.

Dr Duncan considered the paper a valuable and practical one. There were many points in connexion with the treatment of popliteal aneurism that were of great interest. The question of the efficacy of Reid's method was of importance. So also was the question of its mode of action. In one instance compression had been made by it for the remarkable period of six hours with no appearance of gangrene. But coagulation did not take place. In another instance compression by tourniquet had been made over the abdominal aorta for twenty minutes, and coagulation resulted. Such facts indicated marked hæmic differences, and also perhaps that they might carry Reid's treatment further than was usually considered safe. Though the six hours failed in the case referred to, the patient was again put under the treatment for two hours, followed by a day's digital compression, and was cured. To a certain extent he should be inclined to agree with Mr Annandale in applying the old operation—to traumatic cases, for example. A most interesting case was that to which Mr Annandale had referred. It was one in which he had assisted, and Mr Annandale had tied both vein and artery. It was probably unique at the time. In some other instances he should also agree with Mr Annandale; for example, such an one as that he related in which the aneurism interfered with the circulation both to and from the limb. He was inclined to doubt its propriety in suppurating aneurism. He would open the sac if an abscess, but would tie the vessel at a sound point above it, and if feasible dissect the aneurism completely out. Mr Annandale had been hitherto fortunate in his employment of the old operation, but he must not expect to be always so. He would meet with a difficulty sometime. Mr Duncan had assisted Mr Syme in some of these operations, and most formidable they were. In one case, which ought to have been diagnosed as a fusiform aneurism, but was not, an incision of thirteen inches had to be made before the two ends of the artery were laid hold of and tied. It was not always possible to say in popliteal cases whether the aneurism was sacculated or not.

Mr Bell said that he had been anticipated in much of what he wished to say by Mr Duncan. He had assisted Mr Syme at the case referred to, and it was a most formidable affair. Mr Annandale had modestly omitted to mention in his paper that the idea of using bougies to insert in the arterial openings into the aneurism was his own. This was a very great help in the performance of the old operation. He did not agree with him in recommending the operation in cases where the knee joint was opened. He thought that amputation would have a better result. Otherwise he was inclined to agree with him. He thought, however, that

they must remember the effect of the personal equation in all these operations. If the surgeon knew the parts he was dealing with in the popliteal space, he might do the old operation, but if not, it might be a much safer thing for him to stick to the other method, —ligature of the femoral.

The President mentioned an awkward case which had come under his cognisance of abdominal aneurism where pressure had resulted in a fatal rupture of the aneurism.

Mr Annandale said there were one or two cases on record in which Reid's method applied for two or three hours had caused gangrene. In regard to the case of arterio-venous aneurism mentioned by Mr Duncan, he thought that if he had another case of the same he would adopt a plan he had seen practised by Sir Joseph Lister, in which he tied the artery, but sewed up the aperture in the wall of the vein, and so did not interfere with its canal. There was thus less risk of gangrene. He did not think he would care to treat a suppurating aneurism in the way suggested by Mr Duncan, dissecting it out entire would be a very formidable operation. It would be easier to ligature above and below the opening in the artery, and treat the sac as an abscess with careful antiseptic precautions. With regard to what Mr Bell had so kindly remarked there could be no doubt that the introduction of a bougie into the opening in the vessel made the performance of the operation very much easier. He could not agree with him that amputation was best when the knee joint was implicated, because the implication of this joint was usually only by pressure causing some absorption of the bone or other textures. If the sac were laid open with antiseptic precautions the joint need not suffer from the operation. He was also of opinion that the average surgeon would find the old operation much easier than ligature of the femoral artery.

2. *Dr Duncan* read his paper on TRANSFUSION OF BLOOD IN PRIMARY AMPUTATIONS, which will appear in a future number of this Journal.

Dr James said that Dr Duncan's experience of forcibly injecting the blood reminded him of some experiments done in Ludwig's laboratory, in which it was demonstrated that the addition to or abstraction from the amount of blood in an animal's body did not permanently alter the blood pressure. If the experiment were done slowly, the blood pressure had in a short time regained the normal. If it were done hurriedly, there was a danger of extravasation and an unsuccessful result. This suggested the question whether the blood used for transfusion in the case of primary amputation might not have been got in another way, by confining it in the limbs. If, for example, there were a pound of blood in each limb, it might be driven from there when the operation was over by means of Esmarch's bandages. The same effect might be got as was by the injection of blood into the vein.

Mr Annandale, as one who had worked much at transfusion, considered Dr Duncan's paper of very great value. When Roussel brought out his method, he got his instrument, but found it much too complicated, and though he succeeded in two cases, in others he failed. Dr Duncan's method had the merit of simplicity. The operative details only required a little attention, and were most effective. A very important point was the using of the patient's own blood. Some of the saddest cases they had to deal with were those primary cases, in which very little blood was left in the body, and where applying bandages to the arms or leg was of little use.

Dr James said it was impossible to remove all the blood from the body before death. A considerable quantity of fluid would be left in the vessels and intercellular spaces, which might be used in the way he had suggested.

Dr Cotterill had seen transfusion done by many methods, direct and indirect. The operation as performed by the instruments of Roussel, Collin, Aveling, and others was uncertain, complicated, and unsafe; while by the indirect method there was the loss of blood consequent on defibrination. The highly unsatisfactory nature and results of the operation by the above methods had determined him to try the method recommended by Hicks, of adding phosphate of soda solution to the blood. He had employed this method some time ago in a case of gastric ulcer from which there had been profuse hæmorrhage; and although at the time of the operation the patient was almost in "articulo mortis," she rallied very well after the transfusion of about eight ounces of blood. The details of the operation were precisely similar to those employed by Mr Duncan, and the care and safety with which it was performed so struck him that he strongly recommended Mr Duncan to employ this method when about to operate on his case of pernicious anaemia. The operation was now devoid of the two chief risks, namely, those of introducing a clot or a large quantity of air into the bloodvessel. Dr Cotterill did not consider it a *sine qua non* that the blood should be rendered aseptic, as the inherent vitality of the blood would be more than sufficient to take care of any organisms that might be introduced with the blood into the system. At the same time, it was of course wise to keep the wound in the vein, etc., free from septic infection.

Mr Miller said that Dr James's suggestion had already been carried out in practice. They were much indebted to Mr Duncan for bringing the details of this very simple and successful method of transfusing before them, and he hoped that when he published it he would go into the minutest particulars for the sake of those who had no opportunity of seeing the operation done. There were many little points that made for success or failure according as they were properly or improperly attended to.

Mr Bell said that these cases of primary amputation were very

often fatal from another factor than loss of blood. In his last case there was comparatively little of that. The patient had been injured at Portobello Station. A tourniquet was at once put on by a man who had been trained in an ambulance class. It had acted admirably. But during the operation, and for twenty-four hours afterwards, his condition was a most anxious one. During the operation itself he was twice stopped, the chloroformist thinking the patient was dead. He appeared to suffer from what was called shock. What this condition was they did not know, but he believed that in many of the cases a clot formed in the heart and aorta, and if this persisted the patient died. In such cases he did not think transfusion would help, but in those where loss of blood was the cause of the patient's condition it would, and they were much indebted to Dr Duncan for having brought the details of this simple method before them.

Mr Hare asked if any use could be made in transfusion of the extract of leech which Dr Haycraft had discovered to be capable of retarding coagulation for a considerable time. He had repeated Dr Haycraft's experiment, and with six minims of a watery extract had kept two ounces of blood fluid for about ten hours. This extract might be of use in preventing such coagulation as Mr Bell had referred to. There was doubtless an advantage in injecting the blood in an aseptic condition, but it was not absolutely necessary, as large numbers of *bacterium termo* had been introduced repeatedly into the circulation of living animals without producing ill effects. The vitality of the blood seemed to give it strong antiseptic qualities.

The President said he had received a communication from a former pupil in South America, in which he stated that the people in his neighbourhood had great faith in drinking the blood of living animals. Some of them died after such draughts, and the stomach and duodenum had been found full of blood-clot.

Dr Duncan said that shock and loss of blood very often complicated each other in primary cases. He was not prepared to enter on the question of the nature of shock, but he thought it might not be far removed from loss of blood, a bleeding into those abdominal veins to which Dr James had referred. He had no doubt that they would be able to save life in many cases by transfusion. As to the plan or method by which it was to be done, that was subject to experiment and improvement in the future. Operators would require to be careful as to the saline solutions that might be used to keep the blood fluid. Some of these, for example the nitrates, were absolutely lethal. No doubt Drs Cotterill and Hare were correct in their statements that the entrance of a few germs into the circulation did not much matter, but Chauveau's experiments showed the danger if there were local injury. The entrance of air, except in large quantity, did not do harm. It was of importance that the instruments should be

aseptic. In most of the experiments which had been made, the operation had been followed by a rigor, and some even considered the rigor as evidence of a satisfactory injection. It might, however, be that the rigor was due to the entrance of small coagula. In his cases it had not followed, because his method avoids both septicity and coagula. He injected the blood at the temperature of the body.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XLVII.—MEETING II.

Wednesday, 9th December 1885.—Dr HALLIDAY CROOM, *President, in the Chair.*

I. *The President* showed—1. A FIBROID TUMOUR, removed at the end of the third stage of labour. The patient had been under his care during the previous summer, suffering from profuse menorrhagia. All attempts to dilate the internal os by means of sponge tents, etc., had failed, further than merely to enable a small fibroid to be felt attached to the uterine wall, apparently about the middle of the cavity. The patient was sent home, and returned the previous week, giving evidence of being at the fourth or fifth month of pregnancy. Labour pains commenced on Sunday, the 6th inst., and the patient was delivered of a four months' foetus in the evening. After removal of the placenta the hand was introduced, and the fibroid twisted off by means of volsella. 2. An OVARIAN TUMOUR, removed that morning. The tumour was adherent in all directions from recent and acute peritonitis. There was considerable ascites. The tumour was multilocular and semisolid.

II. *Dr Angus Macdonald* showed—1. A DERMOID TUMOUR which he wished to draw attention to on account of (1) its large size, (2) the *thinness* of its walls. 2. The OVARIES removed from a case of bleeding fibroid. The position of the uterus and fibroid rendered their removal impossible, and the ovaries were accordingly taken out. The right one was large and cystic, and was almost like a spleen when removed. Had it not been taken away, the patient would thus have had an ovarian tumour complicating her other ailment.

III. *Dr Milne Murray* showed a series of TRACINGS obtained from the uterine muscle of the rabbit, illustrating the effect of the application of water of different temperatures on that tissue. The apparatus for recording the action was described at a previous meeting. The results obtained were briefly summarized as follows:—1. The normal unimpregnated uterus of rabbits is the subject of rhythmic contraction (1 contraction in about 120 seconds). 2. Water at 105° F. to 115° F. applied directly to the

uterus causes an immediate and prolonged tetanus, lasting for a varying period of from five to thirty or more minutes. 3. This is accompanied by a simultaneous contraction of small bloodvessels, and consequent blanching of the tissue. This vasomotor tension yields before the muscular spasm, but does so quite gradually, and is not followed by reactionary dilation. 4. The application of water at 32° F. to 42° F., after a latent period of 30 to 70 seconds, is followed by a slowly developing tetanus, which continues for a much shorter period than that produced by warm water, and is relatively less intense. 5. The bloodvessels contract, and blanching occurs, but is almost immediately followed by violent reactionary congestion, the organ frequently becoming bright scarlet. 6. The tetanus can be reproduced frequently and immediately by the application of hot water, but can only be reproduced after a distinct interval by the application of cold water. 7. No such tetanus as that described can be produced in the uterus of the rabbit by the faradic current, however strong. Any contractions produced by this agent are slight compared with that produced by either hot or cold water, and invariably cease with the application of the stimulus. These observations will form the subject of a more extended communication at an early meeting of the Society.

IV. *Dr Church* showed a SIMPLE MEANS OF WASHING OUT THE BLADDER, by means of a glass funnel attached to an ordinary catheter by means of a long flexible tube.

V. *Dr Freeland Barbour* introduced a discussion on MICRO-ORGANISMS IN RELATION TO PUERPERAL FEVER, WITH SPECIAL REFERENCE TO ANTISEPTICS. Of the many aspects of puerperal fever which might call for consideration, we propose in this discussion to take up one which has only in recent years begun to attract attention, and which has not as yet been discussed by this Society, viz.,—*Puerperal Septicæmia in relation to Micro-organisms*. For reasons which will be immediately evident we cannot discuss this question without considering the cognate one of *Antiseptics*; and it is hoped that by combining the Scientific and Practical aspects of this question, we shall promote a good debate.

Before looking at these two aspects, we must define the subject and purpose of this discussion. The term Puerperal Septicæmia is given in the title to exclude the consideration of other fevers complicating the puerperium and often included under the broader name of Puerperal Fever. With regard to micro-organisms, we do not propose to enter into the wider question of the validity or non-validity of the germ theory in itself:—Whether micro-organisms be the cause or the result of the morbid processes with which they are associated; and if they are the cause, how it is that they operate? Our purpose is to show that the evidence

goes to prove that *Puerperal Fever stands in the same relation to the Germ Theory as other forms of septic infection do.*

The evidence for this is derived from two sources:—(a.) Scientific investigation; (b.) The use, in practice, of Antiseptics. We shall glance briefly at these two points, and then draw some conclusions.

(a.) SCIENTIFIC INVESTIGATION.—Looking at the subject from this point of view, we note the following facts. The discovery by Pasteur in 1857 of the lactic-acid ferment led to the investigation of the relation of micro-organisms not only to various forms of fermentation, but also to infectious diseases. In 1869, puerperal fever was investigated by Coze and Feltz, and by Mayerhofer, all of whom found micro-organisms present in the blood and in pus from serous cavities. During the last fifteen years various investigators have worked at puerperal fever in relation to micro-organisms, the most important contributors to our knowledge being Pasteur himself, Doléris, Chauveau, Fraenkel, and Lomer. In the course of these investigations various forms of micro-organism have been described in connexion with puerperal septicæmia:—(1.) A monococcus, consisting of isolated points; (2.) A diplococcus, in which the points appear always in pairs; and (3.) Chain-like micrococci, in which the points appear in chains of several members; (4.) Of the chain-like form, Doléris believes he has found a variety distinguished by the larger size of the points of the chain; (5.) Rod-shaped bacteria have also been described. The chain-like form is the one which has been found in the largest number of cases. Last July, in a paper which I read to the Society on the Pathology of the Post-partum Uterus, I described a case in which I had found the chain-like form in several of the organs after death. In that case the blood was not examined during life.

But the *discovery of micro-organisms in the tissues after death* is only the first of the four steps in the process of demonstrating the relation of a micro-organism to a given disease. Koch has formulated these as follows:—(1.) The micro-organism must be found in all cases of the disease, and in that disease only; (2.) It must be isolated and cultivated, and yet preserve its characteristics through a series of cultivations; (3.) Inoculation with a pure cultivation must produce in the inoculated animal the symptoms of the disease; (4.) The micro-organism must be found in the tissues of the inoculated animal, and in the same relation to them as in the original disease.

It is evident that during life, *cultivations* can only be made from the lochia or the blood, unless in exceptional cases, such as Lomer's case of pemphigus vesicles. Results obtained from the lochia carry little weight, owing to the impossibility of excluding germ-laden air. Cultivation of the blood gives in a large

proportion of cases negative results. In a case which came under my notice recently I obtained blood¹ on four successive days previous to death. Cultivation in meat jelly gave negative results. Doléris accounts for this fact by stating that it is a law regulating the development of these organisms that they must be in a condition of rest; they never multiply in the circulating blood, are perhaps destroyed by it. If this be true, whatever tends to a coagulation of the blood will produce a condition favouring their multiplication.

Cultivations have been made in a large number of cases from the tissues and fluids after death. These have generally given the chain-like micrococcus. Pure cultivations of it have been obtained for several generations; and it has been shown that the media of cultivation influence the activity of the virus. Results from the *experimental inoculation* of animals are still inconclusive—some animals, *e.g.* guinea-pigs, being proof against it. Chauveau and Fraenkel have, however, both succeeded in artificially producing septicæmia, sometimes acute and sometimes of a chronic type.

In this brief sketch we have merely indicated the facts obtained from scientific investigation. Space prevents our considering them more fully. A careful examination of them will return a verdict of 'not proven from insufficient evidence' as far as concerns the etiological relationship of micro-organisms and puerperal fever. Still, the evidence points towards our yet obtaining a proof such as we have in the case of splenic fever.

(b.) RESULTS OBTAINED IN PRACTICE FROM THE USE OF ANTISEPTICS.—It seems to me that the germ theory receives remarkable support from the fact that *those substances which have been shown to be most effective in destroying micro-organisms outside of the body have proved to be most useful in treating septicæmia.* There is no obstetrician who does not wilfully shut his eyes to the evidence but now believes that the washing out of the uterus with antiseptics is followed by the most beneficial results. Even with regard to the respective value of different antiseptics, it is interesting to note that Tarnier had satisfied himself clinically of the superiority of corrosive sublimate over carbolic acid before Koch had demonstrated it experimentally. Koch's scientific evidence in favour of corrosive sublimate is so strong that we believe it will replace carbolic acid in obstetric practice. He found that the spores of anthrax bacillus would still grow after

¹ In doing this, the skin must be first thoroughly cleansed with an antiseptic, and the surface pricked with an aseptic needle. The blood is collected in an ordinary vaccination tube, which has been sterilized by heating it in the flame. One end is sealed and the tube moderately heated; when the open end is dipped in the drop of blood the blood of course runs up the partially exhausted tube, the open end of which is then sealed.

immersion for seven days in a 2 per cent. solution of carbolic acid, as also they did after immersion for a day in a 5 per cent. (1-20) solution. But after immersion in a solution of 1-10,000 of corrosive sublimate for from 5 to 60 minutes the same spores became sterile; in fact, immersion for ten minutes even in solutions up to 1-20,000 sterilized the spores. He places the limit of the action of the sublimate on the spores of anthrax bacillus as lying somewhere between a 1-20,000 and a 1-50,000 solution. His experiments on mice were very interesting. Three spore-laden threads were dipped for ten minutes in solutions of 1-10,000, 1-20,000, 1-50,000 respectively, and then introduced beneath the skin of different mice. The 1-50,000 mouse died next day, as rapidly as if the spores had been fresh. The 1-20,000 one died on the fourth, the 1-10,000 on the fifth day. These last showed, therefore, an extraordinary prolongation of the period of incubation, which may fairly be attributed to the action of the sublimate. The same experiment was repeated, with the difference that the spores lay for one hour instead of ten minutes in the solutions. The 1-50,000 mouse died in forty hours, the 1-20,000 mouse died in three and a half days; the 1-10,000 mouse survived. "*Sublimate is therefore the only one of all recognised antiseptics which possesses the very important peculiarity, that it kills by a single application of a comparatively weak (1-1000) solution for a few minutes all, even the most resistant, spores of micro-organisms; even with a solution of 1-5000 a single dipping was sufficient.*"

The fact that it admits of being used in such weak solutions makes it a much more portable article than carbolic acid, a point of some importance to the obstetrician. He can carry a large quantity of it in a concentrated form. Here I show you a convenient solution which Dr Hart got Messrs Duncan & Flockhart to prepare. The solution is of such a strength that one drachm (measured by this glass cup attached to the stopper) added to a quart of water gives a solution of 1 in 2000.

So much for the evidence derived from the use of antiseptics.

In concluding this contribution to our discussion, I would draw the attention of those who accept the germ theory to some points in the development of micro-organisms which I think throw light on some facts which we observe in obstetric practice.

1. The growth and activity of micro-organisms is dependent on the media in which they are cultivated—'media' including soil and other conditions, such as presence or absence of oxygen. This is seen both in the results of artificial cultivation and of experimental inoculation. Arloing's investigations bring out the effect of change of media in a very striking way. He found that when the micro-organisms were transferred to fluid prepared from chicken they lost their activity, but regained it when they were

retransferred to beef bouillon; and that, while inoculation with the most virulent form caused poisoning without suppuration, inoculation with the attenuated form (that prepared from chicken bouillon) produced slow changes ending in abscess formation. The discrepant results of inoculating animals with pure cultivations may be due to the fact that the change of media necessary to obtain a pure cultivation has altered the properties of the microbe. Further, there remains the curious and interesting fact that the tissues of certain animals will not allow certain micro-organisms to flourish in them: Chauveau and Fraenkel found the guinea-pig to be proof against inoculation with the virus of puerperal septicæmia.

These scientific facts give us a basis for this proposition that—*The condition of a patient has a leading rôle in the development of septic poisoning: the nearer the patient's health approaches the normal standard the less is the need of antiseptics.* This is a trite fact in regard to the development of morbid processes generally; but its significance in the case of septic infection has not been sufficiently insisted on. The more nearly the patient's condition approaches that of normal health the greater is the germ resisting power of the tissues, or, in more scientific phraseology, the less favourable are the media which they afford for the growth of microbes. In gynecological operations, especially where the patient's general health is approximately normal (where it has not been seriously affected by the local condition calling for operative treatment), cold water is all very well; but the puerperal patient is so much below par that we cannot trust to cold water or Condry's fluid, but must use the most reliable antiseptic.

2. Micro-organisms require the condition of rest for their development; they will not multiply in the circulating blood. Doléris calls attention to this fact as explaining why, in cases recorded by himself, Pasteur, and others, micro-organisms were not obtained from cultivation of the blood during life, but were readily prepared from cultivations of the blood and other fluids post-mortem; and were likewise found in the tissues, so as to leave no doubt of their having been present before death.

We are not yet in a position to say whether in cases of puerperal septicæmia we have to do with *septic infection* alone, or whether *septic intoxication* may not also occur. In the majority of cases which have ended fatally, and in which the organs have been examined, micro-organisms have been found—pointing towards the existence of a virus which multiplies in the body, like that of a specific fever. That this virus is *specific* there is no proof. Lomer points out that similar chain-like micrococci have been described in scarlet fever, erysipelas, and pyæmia. The fact that they do not multiply in the circulating blood explains, I think, why the washing out of the uterus is followed by such good results; for one's first doubt naturally is how the washing out of

the uterus can prove beneficial, if rapidly-multiplying microbes, producing symptoms of septicæmia, have already passed into the system. The answer to this question is, I think, *that in these cases the toxic material does not multiply in the blood, but is generated in the uterus alone*, from which it is served out into the system, and eliminated by the excretory organs. The relation might be illustrated from the gas supply of a city; in which the gas is, of course, produced in the retorts at the central work, and is simply served through the pipes and eliminated at the burners. By washing out the uterus we put out the retorts and stop further production; and, in a healthy constitution, the elimination of the poison is only a question of time.

There are, of course, cases in which the local process has gone beyond the uterus, *e.g.*, where we have septic thrombosis in the veins of the broad ligament; in this case the production of toxic material may not be limited to the uterine cavity, and therefore the washing out of the uterus will not cut off all the supply of the poison. Or again, we may have the micrococci multiplying in the *lymphatic system* or in the *Fallopian tubes*. In these cases, also, washing out the uterus will do no good.

3. The microbes described in septicæmia differ from those characteristic of putrefaction. In the great majority of cases of puerperal septicæmia, the micro-organism found has been the chain-like micrococcus; the rod-shaped bacteria, common in putrefying matter, has rarely been found in the blood and tissues of the body, although it is not uncommon in the lochia. May we not draw this practical inference, that we may have in the uterus septic matter which is not putrid, and therefore showing no fætor; and formulate the proposition that while *all putrefaction within the uterus after delivery may cause septic poisoning, not all septic poisoning implies putrefaction*. We merely throw out this possible explanation of those cases in which we have septicæmia without fætid lochia, and ask whether we are not bound in those cases also to give the patient the chance of washing out the uterus.

Professor Simpson said: No subject can occupy more profitably the time of the Society than a consideration of the genesis of septicæmia, and I am glad to see Dr Hare here, who will be able to give us further information on the scientific side of the question. I should like to ask Dr Hare to give his opinion as to the following points—Whether we are to look for the cause of septicæmia as being always a specific germ which had to be somehow introduced into the mother's genitals, or whether we are now to admit that other poisons, such as that of diphtheria, erysipelas, etc., if introduced into these passages, may cause a similar condition. Gusserow has, of course, shown that a woman exposed to these poisons may be the subject of attacks quite different from anything we can call septicæmia, but would these poisons, if introduced into the genital canals, give rise to such attacks, or to

the more definite symptoms of what we call septicæmia? Such considerations are interesting in relation to such cases as those recorded by Dr Hunter, who had a series of septicæmias after having attended a patient suffering from erysipelas on the hand. Did these cases run a course similar to a typical septicæmia? Again, it is important to determine whether the poison is introduced only by means of fingers, instruments, apparatus, etc., or if the introduction may not sometimes depend on the germ-laden atmosphere. In the latter case there would be danger whenever a genital wound was exposed; or the microbes might be introduced through the lungs or stomach, and exert their deleterious influence when they came upon the uterine surface or on tissues of lowered vitality. It is well to bear in mind that easy labours are, as a rule, seldom followed by septicæmia; and Winckel has shown that it is exceedingly rare in precipitate labours. This is a point of much interest. Possibly it may be that the suitable media for the cultivation and development of the poison is absent in these cases. We will all agree with Dr Barbour, that we should have early recourse to washing out the uterus in all cases where we find signs of the disease. I agree further with him that corrosive sublimate is the best solution for this purpose. I cannot be deceived at the same time into supposing that the use of this substance is entirely devoid of risk. Thus, in the Maternity Hospital during the present quarter, a patient who showed marked signs of septicæmia improved rapidly under regular uterine douching with corrosive, but on the fourth day had bloody stools, owing undoubtedly to the absorption of the mercury. But we must be content to know this; and we will best avoid it by douching the uterus with plain hot water after the antiseptic has been applied.

Dr Angus Macdonald doubted if he could add much to what had been so well put by Dr Barbour. He admitted that the subject was one of the highest importance. He handed round a drawing of the lochial discharge as seen under the microscope, which had some bearing on auto-infection. He made a series of observations on the condition of the lochia with reference to micro-organisms some time ago. He took great precautions in collecting the fluid so as to prevent the contamination of it by germs floating in the air. The specimens for examination were collected under the spray with every care, and in every case it was found that the cultivations gave an abundance of micrococcus, and that in fluid removed even from the cervix. He was of opinion that the ordinary lochia might itself provide the poison, a view which was held by the old Greek physicians. He certainly agreed with Dr Simpson in thinking erysipelas might give rise to a condition in all respects identical with that of ordinary septicæmia. He recollected a case in which a patient was seized with erysipelas post-partum. The mischief began in the head, and extended downwards over the body. No symptoms of septicæmia whatever were to be found until the disease reached the vulva, when a fatal

result occurred with great rapidity, and with all the signs of acute septicæmia. He heartily concurred with the latter part of Dr Barbour's paper—that bearing on the practical questions between micro-organisms and the use of antiseptics. The relation of these organisms to the production of puerperal fever had been clearly established, and, moreover, we could completely destroy them with corrosive sublimate. In this way we could be sure that we were not carrying them from one patient to another. We might not be able to abolish the disease entirely, as it was likely sporadic cases would occur in spite of all precautions, but we could be quite sure of not infecting our patients. Thus in the Maternity Hospital he (Dr Macdonald) had attended during four trimestres, and had had no case of death from this cause, and, indeed, only one death from any other cause. This result had followed the careful adoption of antiseptic precautions in this Hospital, and was a condition which compared with the most favourable of private practice. He quite agreed with the importance and value of washing out the uterus with corrosive sublimate solution, but reserved this where real danger was threatened, as evidenced by the gravity of the constitutional symptoms. In simple cases he merely washed out the vagina. He was exceedingly pleased with Dr Barbour's paper. He was convinced that a conscientious man had now the means at his command of obtaining an assurance of safety against conveying this disease from patient to patient.

(*To be continued.*)

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING II.

Thursday, December 10, 1885.—Dr LOCKIE in the Chair.

Dr Lockie read a **PRESIDENTIAL ADDRESS**.—After thanking the members for the honour they had conferred upon him, and paying a tribute to his predecessor in the Chair, he glanced briefly at a few of the changes which had occurred in the conditions of medical life and practice during the last twenty-seven years. Having passed in review some matters of local interest, he noticed a number of pathological conditions with which the profession at large had become acquainted within the time named, also a number of new remedies which had been introduced into general use, and which were likely to remain of permanent value. He then proceeded to speak of new instruments for attaining precision in diagnosis and for treatment, with which we had been furnished during the same period, mentioning the clinical thermometer, concerning which it might not be strictly accurate to speak of its introduction as being recent, it having been actually employed in measuring the height of fever by De Haen and Currie at the end of the last century, and by Christison in the epidemic of relapsing fever of 1817-19 in Edinburgh. Indeed, Sanctorius (1561-1636) "had

had the idea of investigating the temperature of the human body in health and disease" by means of the thermometer. Its employment did not, however, become general until recent years. Among the other instruments named were the laryngoscope, the ophthalmoscope, the pneumatic aspirator, the sphygmograph, the hæmacytometer, and the hæmaglobinometer. The President then went on to say, "In no department of medicine have there been greater and more striking changes than in the realm of pathology, and in this domain nothing more noteworthy than the rise and development of the germ theory of disease, taking this term in the restricted sense that many diseases are associated with, and probably caused by, the introduction into, and development and reproduction in, the blood and tissues, one or both, of microscopic organisms. To this doctrine we owe the antiseptic system of surgery, which, whatever be its ultimate outcome, has beyond all question had a mightily beneficial influence in the treatment of wounds, and has immortalized the name of its illustrious founder. . . . To this germ theory we owe the introduction into general use of many antiseptic remedies of which physicians have not been slow to avail themselves, though, it must be confessed, with, as yet, a very imperfect result. But further, may it not be maintained that not only has this doctrine furnished us with new remedies, but it has thrown a ray of light upon the action of old and well-established ones. Physicians have been using antiseptic remedies with success without being aware of their antiseptic properties. Is anything better established than the value of mercury in syphilis? Is not syphilis associated with a microbe? Does not the syphilitic poison in all probability either consist of this microbe, or of something produced by it in the course of its life history? Is not mercury one of the best, if not the very best antiseptic known? There is a remarkable consensus of opinion upon this point. Sir Joseph Lister found a solution of the perchloride one of the best antiseptics. The most advanced accoucheurs use it constantly. Sormani and Brugnattelli, in the course of their experiments, found it, amongst thirty-six substances, the most destructive of tubercle bacilli in phthisical sputum. Miquel, who has made the most extensive observations on micro-organisms, places the biniodide of mercury at the very head of the list of antiseptics, numbering ninety-one, whilst the perchloride is fourth on the list. He found that 0.025 gramme (about three-eighths of a grain) of the biniodide was sufficient to prevent putrefaction in a litre of neutralized *bouillon*, and that 0.070 gramme of the perchloride effected the same object. Can anything be more reasonable than the assumption that when mercury benefits syphilis, it does so in virtue of its antiseptic qualities? May not the same be said of iodide of potassium, which, though an antiseptic, is many times more feeble as such than mercury, and which, though effectual against some of the manifestations of syphilis, has long been thought powerless thoroughly to eradicate the syphilitic poison?

"Take another well-established remedy. Is anything more certain than that quinine does good in malarial fever in the great majority of cases? Now, there is considerable evidence to show that marsh fevers are connected with the presence of a microbe, and it has been proved by experiment that quinine, in solution of a certain strength, entirely prevents the development of certain bacilli. Can anything be more reasonable than to assume that in doing good to ague quinine acts in virtue of its antiseptic properties? Certainly the assumption is far from an unwarrantable one, and is supported by the fact that arsenic, the other great remedy for marsh fevers, is also an antiseptic of proved power.

"What, again, are the remedies upon which we mainly rely in puerperal septic conditions? Are they not quinine and alcohol, both antiseptic agents?

"In gonorrhœa, practitioners of the present day employ antiseptics locally, knowing what they are aiming at, and with success. The old treatment by internal administration of copaiba balsam and cubebs finds its justification and reason for its success in the fact observed by Oppenheimer,¹ that whilst these substances are not directly poisonous to the gonococcus, when they are given internally, substances which are so poisonous arise in the organism and are excreted in the urine.

"In purulent ophthalmia, also, the older practitioners employed with success remedies which are now known to have antiseptic properties.

"Let these examples suffice to show that some remedies formerly used empirically obtain an explanation of their action from the germ theory of disease."

The speaker then referred to the question of immunity from, and the non-recurrence of, certain infective diseases, and quoted, as throwing a ray of light upon these facts, an account of the experiments of M. Raulin on the cultivation of the *Aspergillus niger*, as found narrated in a small work on "Fermentation," written by Dr Duclaux for the Council of the Health Exhibition. These experiments show how minute modifications of the soil are sufficient to diminish, or altogether prevent, the growth of some of these lowly organisms.

Having then narrated the history of the formation of the two medical societies of which Carlisle is the centre, the "Border Counties Branch of the British Medical Association," and the "Carlisle Medical Society," the President concluded his address by appealing to the members to maintain the Society in prosperous and vigorous life by all contributing their individual energy for the common good.

Dr Lediard read a paper on HERNIAS, which will appear in a future number of this Journal.

¹ Quoted by Dr Angus Macdonald, *Edinburgh Medical Journal*, Dec. 1885.

Dr Barnes read a note of SCARLATINAL ALBUMINURIA, which will appear in a future number of this Journal.

Dr Lediard showed Police-Sergeant Roche, wounded by the Netherby robbers. The bullet had passed through the triceps muscle. The wound was healed.

Dr Maclaren showed a CAST of a pair of OVARIES and TUBES removed by operation.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

COMMON SALT IN THE TREATMENT OF PLEURISY.—Schulz has had ample opportunity to confirm a statement made by Robinson with regard to the efficacy of chloride of sodium in the treatment of pleuritic effusions. In a series of cases of acute pleurisy a tablespoonful of a 1 to 30 solution was given every two hours, dry diet being at the same time imposed. Under this simple treatment the exudation diminished rapidly, and the quantity of urine passed was noticeably increased. It is stated that the appetite was improved, and that there was an absence of the thirst which would have naturally been expected. The treatment should be restricted to cases of simple exudation, as it is useless in empyema (*New York Medical Journal*).—*Therapeutic Gazette*, 15th December 1885.

THE INFLUENCE OF CASCARA SAGRADA ON THE DIGESTIVE SECRETIONS.—Cascara sagrada has been experimentally studied by Dr M. Tschelrow (*Centralb. für die Gesamnte Therapie*, August 1885), who comes to the following conclusions:—*First*, Cascara sagrada is useless when a rapidly acting cathartic is desired. *Second*, It only acts as a cathartic when introduced into the stomach; when injected subcutaneously or directly into blood-vessels it produces no evacuation of the bowels. *Third*, Cascara sagrada does not increase the secretion of saliva. *Fourth*, It produces an increase in the secretion of gastric juice, which continues even during digestion. *Fifth*, It increases both the pancreatic and biliary secretions. These results were obtained when the drug was directly introduced into the stomach. When injected into the circulation the animals rapidly passed into a condition of collapse, which soon proved fatal, or they recovered but very slowly; while the blood-pressure was rapidly reduced, even after previous section of the pneumogastriacs.—*Therapeutic Gazette*, December 1885.

BISULPHIDE OF CARBON IN DIARRHOEA.—According to M. Dujardin-Beaumetz, bisulphide of carbon deserves to have a wider therapeutic application than it now possesses. The poisonous intoxicant qualities, which have led to serious accidents during its manufacture, appear from recent experiments by this observer to be minimized by prolonging the process. He has also been led by the evidence which these afford to regard this substance, when well diluted with water, as a valuable antiseptic agent in infectious forms of diarrhoea, particularly in that of typhoid fever. His formula is the following:—

Bisulphide of carbon,	.	.	25 grammes.
Essence of mint,	.	.	50 drops.
Water,	.	.	500 grammes.

The mixture is placed in a vessel of about 700 c.c.m. measurement, shaken, and allowed to stand till all deposit has been thrown down. When any of the fluid is drawn off, care must be taken to add water in like quantity to the remainder. Four to ten table-spoonfuls of this solution should be taken throughout the day. Milk is a suitable medium for administration (*Progrès Médical*, 1st August 1885).—*The Practitioner*, December 1885.

INCOMPATIBILITY OF CHLORAL HYDRATE WITH POTASSIUM BROMIDE AND ALCOHOL.—Professor George F. H. Markol, in experimenting with solutions of chloral hydrate, finds this drug incompatible with bromide of potassium and alcohol. He says,—Experiments prove that the alcohol is the cause of the trouble, and the writer thinks that the chloral hydrate is changed into the less soluble chloral alcoholate. The writer found that the addition of potassium bromide, sodium bromide, sodium chloride, and magnesium sulphate, to strong solutions of chloral hydrate, together with the presence of alcohol, determined a separation of the liquids into two layers. Ammonium chloride, ammonium bromide, potassium nitrate, and calcium bromide, did not disturb the same chloral solutions. The practical lesson to be learned from this incompatible prescription is that alcoholic preparations should not be prescribed with chloral hydrate, especially not in connexion with the bromides of potassium and sodium, because if concentrated solutions are used, the chloral will separate as alcoholate, float on the surface, and a great risk will be incurred of giving a large overdose, the patient having received no caution with regard to the necessity of shaking the contents of the bottle before taking a dose.—*Boston Medical and Surgical Journal*, 23rd July 1885.

NOTES ON THE NEW ANTISEPTICS, HYDRONAPHTHOL AND THE POTASSIO-MERCURIC IODIDE.—In a paper read by R. J. Lewis, M.D., before the Philadelphia Academy of Surgery, on 2nd November 1885, the following are the claims made for the newly-discovered antiseptic, hydronaphthol (*Maryland Med. Jour.*, 2nd November 1885):—It is at least twelve times as effective as car-

bolic acid, and is entitled, as a true antiseptic, to occupy a position in the comparative tables next to the mercuric bichloride. It is 30 times as potent as salicylic acid, 60 times as effectual as boric acid, and has 600 times the antiseptic power of alcohol. Hydronaphthol is soluble when placed in cold water to the extent of 1 part in 2000. It is soluble in hot water, in the proportion of 1 to 100; but when the water becomes cooled to ordinary temperatures a precipitate occurs, leaving a solution of 1 to 1000. In this strength of 1 to 1000 it permanently prevents the development of the germs of putrefaction in all putrescible fluids. Whilst the true antiseptic or *inhibitory* action of hydronaphthol in such cold aqueous saturated solution is perfect, its germicidal and proper disinfectant power is ineffective. For the destruction of already existing germs, such as have a tenacious vitality, as those of anthrax bacilli and pathogenic micrococci, it therefore cannot be relied on. As to its action in this regard, as compared with carbolic acid, it should be remembered that a 10 per cent. carbolic solution is required,—a strength practically improper in wound treatment. In ordinary antiseptic practice carbolic acid is valuable only on account of its inhibitory action. The first use of hydronaphthol as an antiseptic was by Dr H. R. Fowler of Brooklyn, to whom the profession is indebted for its introduction to practical surgery. Dr Lewis's experience with the antiseptic action of hydronaphthol in the wards of the Pennsylvania Hospital, and in private surgical practice, confirms his observations as given in his recent articles in the *New York Medical Journal*. Hydronaphthol is a grayish substance in the form of crystalline lamina, having a slightly aromatic taste and odour. It is soluble freely in alcohol, ether, chloroform, glycerine, neurole, and the fixed oils. In the aqueous saturated solution of 1 to 1000, it is absolutely unirritating, and has no toxic action either local or systematic, is free from unpleasant odour, and has no injurious action on metallic instruments or on clothing fabrics. Besides its use in aqueous solution, Dr Lewis has used it in the form of a powder diluted preferably with the oxide of zinc in the proportion of 1 to 50; and he believes that hydronaphthol may well displace carbolic acid from practical surgery. The potassic mercuric iodide is four or five times as powerful as a local germicide or disinfectant as the mercuric bichloride. For such use it is effective in aqueous solutions, in the proportion of only 1 to 12,000. The potassic mercuric iodide is made by simply dissolving equal quantities of the biniodide of mercury and the iodide of potassium in distilled water. The solution is evaporated, and there remain yellow needle-like crystals of the potassio-mercuric iodide. In the use of such dilutions of this powerful antiseptic local irritation is entirely avoided, and the risk of producing the constitutional effects of mercury is greatly diminished. The introduction into surgical treatment of these two remarkable and powerful substances, hydronaphthol and the

potassio-mercuric iodide, will do much to overcome some of the objections and inconveniences of antiseptic practice.—*Therapeutic Gazette*, 15th December 1885.

ON THE CAUSE AND THERAPEUTICS OF INSUFFICIENT LACTATION.—We abstract the following noteworthy *résumé* of the cause and therapeutics of insufficient lactation from a paper of Dr Dolan appearing in the *Alleg. Wien. Med. Z.* and *La France Medicale* (No. 71, 1885):—1. A therapeutic action on the secretion of milk is only possible through the blood. 2. All liliacæ, cruciferæ, solanæ, and umbelliferæ pass from the blood into the milk. 3. There are no veritable galactogenic remedies. The action of jaborandi is uncertain. 4. Belladonna is an antigalactogenic drug. 5. The proportion of the salts in the milk can be augmented by medicines. 6. Diuretics and purgatives can be administered to the child through the milk of the mother. 7. Dietetic and hygienic means improve both quantity and quality of milk. The causes of insufficient lactation are:—1. Insufficient formation of the glandular utriculi or of the epithelium. In this case treatment is useless. General plethora can be suppressed by appropriate diet and castor-oil. 2. Torpidity of the mammary glands. Electricity, warm poultices, and cupping will relieve this condition. 3. General anæmia and insufficient alimentation. Dolan regards the presence of dental caries as very important in this respect; the country women who usually possess faultless teeth can also nurse their children best.—*Therapeutic Gazette*, 15th December 1885.

MEDICAL PERISCOPE.

By FRANCIS TROUP, M.D.

THE CONTINUOUS ADMINISTRATION OF THALLIN AND ITS EFFECTS IN TYPHOID FEVER.—Drs Ehrlich and Laquer, in Nos. 51 and 52, 1885, of *Berlin Klin. Wochenschrift*, call attention to the virtues of thallin first introduced into practice by Jaksch. They assert that in many cases of typhoid, whose history is given, a perfect defervescence was brought about on an average of four or five days' treatment. Some of them had proved rebellious to quinine and cold baths. They have discovered that profuse sweats, rigors, and collapse, the disagreeable concomitants of dosage with thallin, can be absolutely prevented by using moderate doses very frequently repeated, and that "Thallinization" may be kept up for days or weeks, if necessary, without danger. They call attention to the fact that thallin salts contain very different percentages of the medicine—the sulphate 77, the tartrate 52, and the tannate 33 per cent.; they have preferably used the tartrate, and where diarrhoea was profuse the tannate; it should be given in pill form, as the taste is not agreeable. It is of importance to determine by preliminary trials the personal equation of each

patient with regard to thallin, and the dose discovered which will reduce the temperature from $39^{\circ}5$ to 38° or $38^{\circ}5$, and keep it there for a few hours; this quantum found out, may then be given without fear of unpleasant effects for days or weeks, of course under constant control of the thermometer. It is best to begin with a minimal dose of $\cdot04\text{--}\cdot06$ of a gramme hourly, and increase by centigrams every second or third hour till the dose has been discovered that will reduce the temperature above mentioned. The pills should be given hourly during the day, and every second hour during the night. The authors believe that thallin in this disease has a specific as well as an antipyretic virtue,—1, because of its typical and progressively increasing influence over the temperature; 2, because the cases brought to an end in a few days, so far as temperature is concerned, were not specimens of slight fever, or of those having a tendency of themselves to abort, as evidenced by the continuance, after the somewhat precipitate deservescence, of splenic enlargement, diarrhoeic stools, and diazo-reaction. Trials made of this drug in intermittents and joint rheumatism were not successful, but in erysipelas and pneumonia better results were obtained.

BRAIN SYPHILIS (*Berlin. Klin. Wochenschrift*, No. 1, 1886).—Gerhardt remarks that brain syphilis can show itself in endless forms and combinations, according to the quality and locality of its anatomical sub-stratum. Yet, in many cases, without confession of syphilis or demonstrable traces of it in other organs, one may make the diagnosis. This should be the aim of the physician, and should be more frequently attained than it hitherto has been. The disastrous maxim, to give a little iodide of potass in all obscure cases of brain disease has healed few and been unsuitable to many more. One may enumerate a great number of single symptoms which are very important, but not of themselves demonstrable. To such belong severe diffuse brain disturbances characterized by unilateralness, incompleteness, and incomprehensible inconstancy. In the same category are quickly repeated apoplectic attacks in young people free of heart disease, paralysis of ocular muscles, especially ptosis, epilepsy arising without particular cause in later years, cortical epilepsy, monoplegiæ, acute bulbar symptoms, tumour symptoms which cannot be explained by one focus of disease. The mere enumeration of these and many other isolated symptoms and groups of symptoms is not of much use; it is, perhaps, better to accentuate a few general features. As endocarditis is the most frequent anatomical lesion, apoplectiform attacks are especially often observed. To Gerhardt it seems almost permissible to comprehend apoplexy as symptom, and syphilis as its fundamental cause. In 63 cases (tumours excluded) 13 were certainly syphilitic, and in 9 others suspicion of the same was almost certainty. Strictly, syphilitic apoplexy most frequently arises by the supervention of autochthonous thrombosis in arteries constricted and narrowed by slow changes. This is prefaced by prodromata of a more pronounced

kind than is usually the case in sanguineous apoplexies. The attack may be very complete; it is more typical, however, when repeated frequently in short time and without loss of consciousness. If the terminal twigs of the carotid are affected we have a common hemiplegia; if those of the vertebral, then we have bulbar symptoms. Gummata are quite commonly superficial overlaying tumours (Beleggeschwülste). They may cause the most diverse cortical symptoms and paralyses of the cranial nerves; but one thing they cannot bring about, and that is the general violent headache with deep stupor of massive expansive brain tumours, such as gliomata; when cortical, monoplegiæ, cortical epilepsy, particularly mixed forms of epilepsy and hemiplegiæ, will be brought about; when basilar, crossed paralysis, severe implication of isolated cranial nerves, and so on; ptosis will also play its rôle and occasion frequent vertigo, and in both other muscles of the eye will be paralytically involved. Like syphilitic skin diseases, which metamorphose themselves into each other, and run through the series from roseola to deep gummatous abscesses, so polymorphism is the chiefest attribute of brain syphilis. We have epilepsy, for instance, on which an apoplexy shortly follows, or to which marvellous disturbances of the faculty of thinking, of memory, or of speech are added, or in the reverse order. Matters are often so that one cannot explain the changes by supposing them to emanate from one easily proved focus. Many of those cases are curable if seen and recognised early; later they are not so: one only needs to think of the endarteritis and the consecutive brain softening which follows complete obliteration of the vessels to believe this. The cases must be treated as early, as energetically, and as long as possible. For many weeks 3-7 grammes of Grey ointment must be rubbed in, and 2-5 grammes of iodide of potass taken daily. [Grey ointment, "Graue Salbe," is rather stronger than our Ungt. Hydrarg. Fort.]

PERISCOPE OF MEDICINE, CLINICAL AND PATHOLOGICAL.

By G. A. GIBSON, M.D., and WILLIAM RUSSELL, M.D.

DIPHThERIC FORM OF SYPHILITIC TONSILLITIS. — M. Albert Robin at the Pitié Hospital delivered an instructive lecture on this condition. The patient was a young woman suffering from extreme exhaustion and debility. The temperature was elevated. The throat was inflamed and showed several diphtheritic-looking patches of considerable size, and the glands in the neck were swollen. The case was admitted as a case of diphtheria. When M. Robin saw her the following day he discovered a papular syphilide over the body, and condylomata round the vagina. The question which presented itself in this case was whether it was a case of diphtheria in a syphilitic patient. This was decided in the negative, as her general condition had improved greatly within

twenty-four hours, while at the same time the tonsillar patches had increased in size. Although in this case the diagnosis was comparatively easily made, M. Robin referred to the difficulty in some cases. The points which he laid stress upon for differential diagnosis were—(1.) The position of some of the patches on the hard palate where diphtheritic patches do not usually occur; (2.) Although the “false membrane” had the colour, the thickness, and apparently the coherence of a diphtheritic membrane, it was found to be extremely friable and readily detached; (3.) Ordinary syphilitic mucous patches have an opaline hue, but in this diphtheritic form the patches have a grayish-white colour. He also dwelt at length on the diagnosis of chancre of the tonsil, which appears to be not unfrequently seen on the other side of the Channel.—*Gazette Médicale de Paris*, 2nd and 9th January 1886.

THE PATHOLOGY OF HEMIANOPSIA.—In a contribution to this subject Seguin recognises the following types:—1. Horizontal hemianopsia, superior or inferior, usually due to lesions within the eye, and hence of comparatively little interest to the neurologist. 2. Vertical hemianopsia, always caused by a lesion of the basal visual apparatus, or by disease in a certain limited part of the cerebrum. Its varieties are:—*a.* Temporal hemianopsia, in which the temporal half of both visual fields is dark. *b.* Nasal hemianopsia, in which the nasal half of both visual fields is obscured. *c.* Lateral hemianopsia. This is often designated homonymous hemianopsia. In this variety the nasal half of one visual field and the temporal half of the other are dark, so that, with one or both eyes open the patient sees the same half of any object placed in front of him. This variety is one which results from a truly central lesion, and forms the subject of study in the paper. Varieties *a* and *b* are caused by lesions variously placed in or around the chiasm of the optic nerves. He gives an analysis of forty-two cases, and from their evidence he considers it to be well proved that the visual centre (receiving impressions from one corresponding half of each retina) in man is in the cuneus and adjacent gray matter below it. A destruction of this part of the hemisphere inevitably produces lateral hemianopsia of the field on the opposite side of the body. As regards those few cases in which the lesion causing lateral hemianopsia has been situated upon the lateral aspect of the hemisphere, in the inferior parietal lobule and the gyrus angularis, Seguin calls attention to the fact that the optic fasciculus of Gratiolet and Wernicke on its way from the primary optic centres, laterally to the posterior horn of the lateral ventricle, to the cuneus, passes close under these gyri, and a lesion which penetrates at all beneath the gray matter of their cortex must intercept the optic fibres, by pressure or by destruction. These are lesions intercepting communication between the eyes and the visual centre, while lesions of the mesial aspect of the

occipital lobe destroy the centre itself.—*New York Medical Journal*, 14th November 1885.

HEMERALOPIA (NIGHT-BLINDNESS) IN JAUNDICE.—Two cases in which this condition was observed are briefly recorded. The first, a man, aged 52, had long suffered from cough and dyspnoea, and for two years from pain in the right hypochondrium. There was great ascites, and the liver appeared to be atrophied. Jaundice was marked. The patient had never seen things yellow. There was a cataract in one eye, but with the other vision was normal in daylight, while at night it was so defective that he could not find his way. By lamplight he could distinguish objects with difficulty, and when the lamp was extinguished, although darkness was not complete, blindness was absolute. With the ophthalmoscope the fundus was seen to be of a reddish yellow. At the autopsy the liver was found cirrhotic. The second was a man, aged 51, who had severe pains in the right hypochondrium and jaundice; the liver was enlarged and tender. By day acuity of vision was normal; at night, if the room was feebly lighted, he could not distinguish objects at but a short distance from him. The ophthalmoscope only revealed a yellowish tint of the fundus.—*Wiener Medicinische Presse*, No. 27, 1885, in *Archives Générales de Médecine*, January 1886.

CURATIVE TREATMENT OF ANGINA PECTORIS.—The *Gazette Médicale de Paris* for 2nd January 1886, contains a review of Dr Huchard's communication on this subject. In approaching the subject it is necessary to distinguish clearly between true and false angina pectoris. The latter occurs in persons suffering from nerve diseases, arthritic and gastric affections, and at all ages particularly in women. The frequent repetition, the periodic appearance, the long duration, the site of the pain at the middle or inferior part of the cardiac region, characterize these false anginas. True angina, more frequent in men, comes on at a more advanced age. It assumes but exceptionally the periodic form; it is never spontaneous, but induced nearly always by an abnormal or exaggerated excitation of an enfeebled heart, by an effort or an emotion. Its site is distinctly substernal, and the suffering agonising. While the pathogeny of the false form is variable, the true form is to be explained by an invariable pathogeny,—namely, to lesion of the coronary arteries, to their sclerosis or constriction; most often it is the result of an arthritis, leading to interference with the lumen of the coronary arteries at their mouths; and the paroxysms are induced by a temporary cardiac ischaemia. This exclusively arterial theory is accepted by Potain and German Sée. Acting on this theory, M. Huchard has banished all therapeutic measures which increase vascular constriction, as ergot, or elevate arterial pressure, as digitalis. For the paroxysm he uses nitrite of amyl for inhalation, in from 4 to 10 drops. Morphia given hypodermically augments the force of the heart beats, diminishes the blood pres-

sure, and passively dilates the arteries, and has thus a certain efficaciousness, but is less rapid and less sure. Solution of nitroglycerine may also be used either internally or hypodermically. For the curative treatment the iodides are implicitly believed in, but to obtain results it is necessary to continue them for fifteen or eighteen months, and to take from 15 to 46 grains daily. The sodium is preferred to the potassium salt.

ATROPHY OF THE SUPRARENAL CAPSULES, WITH ENLARGED SPLEEN AND DARK URINE.—Saundby narrates the cases of a brother and a sister who were in the habit of passing dark-coloured urine. When first seen the brother was 16 years old. His urine was porter-coloured, with deposit of urates, oxalates, blood-casts, and broken-down blood-corpuscles. Albumen was present, but no sugar or bile. MacMunn of Wolverhampton examined it with the spectroscope, and found the colour to be due to methæmoglobin and urobilin. His blood had very slight excess of leucocytes. The skin was dusky yellow, but the conjunctivæ were white. The spleen was enormously enlarged. He had the cardiac and vascular signs of anæmia. His father had died at the age of 37 with enlarged spleen ($7\frac{1}{2}$ lbs. on post-mortem examination), dusky skin, and dark urine. The boy himself had always passed dark urine, and his spleen was known to be enlarged. He got over the acute attack for which he was seen, and is now in the same physical condition, except that the spleen is smaller when he is not suffering from one of the attacks. His sister also came under observation, and it was noted that at that time, when she was 16 years old, there were no abnormal physical signs, especially no evidence of splenic enlargement; her complexion was slightly ochre-tinged, like her brother's; her urine was always dark. She menstruated for a short time regularly, but this had stopped for six months. She was in other respects a well-grown, well-developed, and intelligent girl. Her urine was dark amber, turbid, 1015, acid, no albumen; turbidity disappeared on heating; it gave a faint blue colour with guaiacum and ozonic ether. A specimen of the urine of each was submitted to MacMunn, who reported as follows:—
 “No. 1, Sister.—Urine deep yellow or brownish-yellow colour, with mucous cloud at bottom of bottle. Reaction acid, sp. gr. 1012, no albumen; no blue colour with guaiacum and ozonic ether; with nitric acid gives only red and violet. The colour is due to urobilin, indican, and it also yielded onucleolin (so called). It contained no blood-pigment and no bile-pigment.”
 “No. 2, Brother.—Urine the colour of dark tea, depositing a sediment of red brick urates; sp. gr. 1024, acid; on boiling, a cloudiness which was only partially dispersed by acetic acid. On boiling with cupric sulphate and caustic potash a partial reduction took place, the solution becoming olive brown (glucoside—indican), with nitric acid it gave red and violet. With guaiacum and ozonic ether a green colour, but no blue. Its colour was due to indigo-red (as such),

indican (which could be split up into indigo-blue), urobilin, urerythrin, and, strange to say, a trace of a pigment which had a great likeness to cruentin, a pigment got from hæmoglobin and hæmatin by the action of sulphuric acid." The urine from which these reports were taken was passed during intervals of ordinary health, not during attacks. The sister gradually sank, and, in spite of all that could be done, died in a comatose state. The following is the description of the post-mortem examination:—*Thorax*.—The heart showed some excess of fat in the epicardium; there was a pale clot in the right side; the left ventricle was contracted and slightly hypertrophied; the valves were normal; the pericardial cavity was dry. The pleural cavities were dry; both lungs were crepitant and healthy; there was no tubercle. *Abdomen*.—The spleen occupied the whole of the left hypochondrium; it weighed 66 oz.; it was dark purple in colour, hard, and tense. On section it looked like damson cheese. It gave no amyloid reaction. Under the microscope a large quantity of granular brown pigment was diffused through the pulp, but there was no other structural change. The liver was quite healthy in appearance. The stomach was dilated, and its mucous membrane was covered with bile and mucus; it contained half a pint of altered bile. The kidneys were rather large, their cortices looked dark, and the medullary portions pale. Under the microscope pigment could be seen lying in the tubules. The suprarenal capsules were very small, the right weighing 18 grains and the left 10 grains. The semilunar ganglia appeared normal, the right weighing 5 grains and the left 6 grains. Under the microscope the capsules showed no trace of inflammation, or fatty or fibroid degeneration; they *seemed to be simply atrophied*. The semilunar ganglia were healthy, no change being apparent in their nerve-cells, connective tissue, or bloodvessels." Specimens of atrophy of the suprarenal bodies have been seen, but consequent upon some inflammatory process. In this case it was *simple atrophy*. (This communication is very valuable, not only as being related to Addison's disease, but as bearing upon the researches of MacMunn on the functions of the adrenals, which we noticed last spring in the pages of this Journal).—*Transactions of the Pathological Society of London*, vol. xxxvi.

PERISCOPE OF GYNÆCOLOGY AND MIDWIFERY.

By J. MILNE CHAPMAN, Inverness.

THE CONTINUOUS OR SPIRAL CATGUT SUTURE IN GYNÆCOLOGY (Leading Article in *Philadelphia Medical News*, 5th September 1885).—The use of the continuous catgut suture in gynæcology, especially in plastic operations, has, probably, not received in this country the attention it deserves. The remarkable successes, especially in perineorrhaphy and in perineoplasty, that have been attained with it abroad by Schroeder, Brüse, Bar, Veit, and others,

entitle the method to be properly understood and fairly tried. Among important contributions to the literature of the subject may be mentioned one by Bröse, *Arch. für Gynäkol.*, 1883—one by Bar, *Archives de Tocologie*, February 1885—and a discussion upon the subject by the Berlin Obstetric and Gynæcological Society, in connexion with a paper by Schroeder, the report appearing in the *Centralblatt für Gynäkologie*, 18th July. A study of the subject chiefly in connexion with these contributions may be interesting and valuable to our readers. According to Schroeder, Graefe was the first to use the continuous catgut suture in a recent rupture of the perineum, but it was not until Küster devised the method of preparing the catgut in oil of juniper that these sutures dissolved in nine days. Of course, the catgut must be completely aseptic, and in its preparation one has to avoid such changes as will prevent its too rapidly dissolving in the tissues, or its failure to dissolve at all. The latter accident, which has occurred to us in some cases from using the MacEwen catgut, is very trying to the patient and mortifying to the operator. According to Bröse, No. 2 Lister's catgut is a suitable size, but it is not to be prepared according to Lister's method. Apparently the best method of preparing it is, first, soak it twelve hours in Van Swieten's liquor, and then place it in oil of juniper; no definite period is stated for its remaining in the oil of juniper, but probably a few hours will be sufficient; afterward it is preserved for use in absolute alcohol, and immediately before being used is washed in a 4 per cent. solution of carbolic acid. Schroeder thinks it of very great importance not to irritate the denuded surfaces in a perineoplasty in the process of cleansing them, and he is now using for the latter purpose only distilled—that is, sterilized—water. Veit irrigates these surfaces with a 1 to 10,000 sublimate solution. The method of denuding the surfaces is immaterial, only care must be taken not to carry the denudation too far on the sides lest undue tension results. After the surfaces have been freshened, the operator, taking a needle threaded with catgut, begins stitching at the upper, the vaginal angle of the wound; after passing the suture from one to the other side, he may either knot or tie the catgut, and he then stitches from side to side, making a continuous or spiral suture along the vaginal surface of the rent to the skin surface of the perineum. He may then continue with the same needle, sewing up the perineum with deep and with superficial sutures; or, for the present leaving that needle and catgut, take another threaded needle, and begin the stitching of the perineum at the posterior portion of the rent, and when he has completed this, by a similar spiral suture, remove the needles, and tie the two pieces of catgut together. Or the same result may be accomplished if only a single catgut suture has been used, since in knotting or tying the first stitch one may leave a projecting piece, and to it the final end is tied, the initial and terminal portions being thus united. In case the tear of the perineum be complete, it is

well to use stitches in the rectum, and these are taken in the same way as the vaginal, but, of course, should be made before the latter. Bar states that some operators regard it as important to knot the suture from time to time; this prevents separation of parts if the catgut should happen to break. So, too, even in the vaginal stitching, if the depth of the fissure be great, it is of essential importance to have a series of deep, and then one of superficial, sutures. One should have three or four needles threaded, so that, if the catgut breaks, there will be no delay in continuing the operation; these needles and catgut are kept in the carbolyzed water mentioned. Schroeder mentions among the advantages of the continuous catgut suture the rapidity with which the operation can be done, the absence of bleeding, and the accuracy with which the torn surfaces can be brought and kept in contact. After the operation, he has the patient's lower limbs fastened together, and keeps her in bed three weeks. To prevent distention of the rectum with fæcal gases, he makes a subcutaneous division of the anal sphincter, and inserts a small tube in the rectum. His results are—of twenty-five operations upon twenty-four patients, all cured; one, as will be observed, had to have a second operation. The results of colporrhaphy have also been much more favourable with the continuous catgut suture; posterior colporrhaphy gave more successes than anterior, the movements of the bladder interfering with perfect healing in the latter,—a difficulty which he has endeavoured to overcome by tamponing the vagina with iodoform cotton. Odebrecht claims brilliant results from this suture, which he has adopted by the advice of Hofmeier; he has operated in three cases of rupture of the perineum, all of which succeeded,—two of them being primary, the other a secondary operation. In one of his cases he used a rectal suture introduced before the vaginal. Veit's cases of perineal operation with catgut suture were seven, and six were successful. Besides, he had used this suture in thirty cases of prolapse operations, and two of vesico-vaginal fistula; the results were good. [Our own experience with the continuous catgut suture in gynecology has not been so great as to justify a conclusive opinion. The first difficulty we met, and which prevented success, was the unsuitable catgut used; some broke at almost every stitch, others split even after threading the needle and while taking the stitches, while still others refused to dissolve. Nevertheless, with catgut of suitable size and quality, and prepared as mentioned in this article, the results have been such as to encourage us to new trials.]

A NEW EXPLANATION OF THE PROCESS OF MENSTRUATION (Löwenthal, *Archiv f. Gynäk.*, xxiv. 2).—The following are the propositions with which the writer concludes his paper:—1. The periodical flow of blood from the female genitals is not a consequence of the rupture of a follicle (which usually occurs at the same time), but of the degeneration of the uterine mucous membrane which has become hypertrophied before such rupture, and

independently of it. 2. This growth, the menstrual decidua, is caused by the imbedding in the unimpregnated condition of the last ovum furnished by the ovaries. 3. If this imbedded ovum is impregnated, the menstrual decidua becomes that of pregnancy, but if the ovum remains unimpregnated, it degenerates as a consequence of the death of said ovum. 4. Taking each menstruation for itself, the rupture of the follicle and the flow of blood stand in no other casual relation to each other than that possibly the causes and conditions which are operative at the occurrence of the hæmorrhage may, at the same time, have an effect in bringing about the rupture of a ripened follicle. 5. The coincidence of the rupture of a follicle and the flow of blood is, therefore, no necessary one. Each can occur separately; a follicle can rupture without there being any menstrual decidua present, and this secondary consequence of the ovum which has previously made its appearance—namely, the menstrual flow—can occur without a new follicle rupturing at the same time. 6. The periodicity of menstruation is determined by the extra-follicular length of life of the imbedded and unimpregnated ovum; the variations from the general or individual periodicity depend upon the shortening or absence of such capacity for life, either idiopathic or the result of intercurrent influences. 7. The ovum which has been expelled, usually at the last menstruation, and is present ordinarily in the uterus (in abnormal cases elsewhere), is the one which is impregnated. From these the following practical conclusions can be drawn:—8. Since the menstrual flow is neither a physiological function, nor the necessary accompaniment of such a function, but the direct consequence strengthened by innumerable repetitions of a process which is the result of our social conditions,—namely, the unimpregnation and death of the human ovum,—therefore it has all the properties and results of other hæmorrhages which are always pathological. 9. It increases and diminishes under like conditions with them. 10. The flow of blood which is the necessary accompaniment of the menstrual decidua, is only to be regarded as harmless when it occurs per diapedesin. When per rhexin, it is under all circumstances unnecessary, and becomes injurious when it is felt and responded to by the entire organism as a loss. 11. The amount of the injury corresponds to the proportion between the amount of blood lost and the quantity (plus the quality) of blood which the body contains at the time. 12. Under these circumstances the indication is for the greatest possible reduction in the amount of the menstrual flow, as of any other loss of blood. 13. The means to be recommended are first and foremost rest in bed, and hot water injections. 14. On the contrary, the voluntary cessation of the menses (idiopathic amenorrhœa) is under no circumstances to be regarded and treated as a disease; it is only a sign that a function (ovulation) which is not necessary to the life of the individual, from some cause or other (youth, age, pregnancy, lactation, debility), was not properly performed at the proper time, that is, about four

weeks previously. 15. Supported by this theory, and the observation of Lawson Tait as to the importance of the tubes for menstruation, it would be desirable to attempt to substitute for castration salpingotomy (partial resection of both tubes after previous ligation), in those cases where uterine dysmenorrhœa, menorrhagia, fibroids, and such uterine affections call for the anticipation of the menopause. 16. Should it appear impossible after beginning the operation for castration in the above-mentioned conditions to completely remove the ovaries, salpingotomy would be directly indicated.

A SERIES OF 112 CONSECUTIVE OPERATIONS FOR OVARIAN AND PAROVARIAN CYSTOMA WITHOUT A DEATH (Lawson Tait, *Ibid.*, 12th Sept.)—The series of cases may be analyzed as follows:—Dermoid cyst, 1; cystic sarcoma, 1; abscess of ovary, 2; cystoma of one ovary, 49; cystoma of both ovaries, 38; parovarian cysts, 21. "In not one of the cases," says the writer, "has carbolic acid, or thymol, or corrosive sublimate, or any of the multifarious germicides touched my patients either directly or indirectly." He attributes his "growing success" to "increased personal experience, increased attention to all the minute details which go to create success, and inattention to any one of which may defeat the best laid plans. In addition to these I may add scrupulous attention to cleanliness, and an undeviating enforcement of discipline, both on the part of the patients and my staff of assistants.

OCCASIONAL PERISCOPE OF THE DISEASES OF CHILDREN.

By CHARLES E. UNDERHILL, M.B., F.R.C.P.E., Physician to the Royal Hospital for Sick Children.

PNEUMONIA IN YOUNG CHILDREN.—Dr Holt, in this paper, gives the results of his experience of 86 cases, drawn from dispensary and private practice. He divides them into two series,—1, lobar; 2, broncho-pneumonia, the latter being equivalent to our catarrhal pneumonia. Of 74 cases under five years of age, 18 were lobar and 56 broncho-pneumonia. 1. The disease is primary, and occurs often in robust children. It is most frequently ushered in by attacks of vomiting. The fever runs high, and there is great thirst. The cerebral symptoms are often so marked as to obscure the diagnosis. Diarrhœa is not infrequent. A positive diagnosis can only be arrived at by the physical signs. The seat of the disease differs from that in adults. The author agrees with Rilliet and Barthez in assigning the following order of frequency: first, right apex; second, left apex; third, left base; fourth, right base. The frequency of apex pneumonias make it of great importance to examine for physical signs high up in the axilla with great care. The physical signs generally resemble those in the adult, but are not so easy to find. Pulmonary congestion occurring in the course

of other diseases is often mistaken for pneumonia. It is impossible by physical signs to differentiate these; the congestion can only be recognised by its mobility and evanescent character, rarely lasting more than two days. The mortality in the cases of lobar pneumonia was very small—only one case in the 25 died.

2. Broncho-pneumonia is usually a secondary disease, occurring in the course of an attack of whooping-cough, measles, scarlatina, or other diseases. It is an inflammation of the mucous membrane and walls of the bronchi, the air-cells, and the interstitial tissue of the lung, the bronchial element predominating. It attacks children in a debilitated condition; its onset is gradual; it rarely terminates by a crisis; and has no typical course. The febrile stage lasts for three or four weeks, and the temperature is very variable. The respirations are very rapid, and the breathing is more laboured than in the lobar form; there is more lividity of the lips and cyanosis of the face; and towards the close of the disease, in fatal cases, a drowsiness comes on, the result of carbonic acid poisoning. The cough is frequent and distressing. Both lungs are usually affected, but to a varying extent. The diseased parts are full of moist râles, but pure bronchial breathing is rarely heard. In bad cases more or less of both lungs may become consolidated. These consolidated patches often take months to entirely clear up; they rarely become the seat of tuberculosis, but may terminate in chronic fibroid induration with dilated bronchi. As regards treatment, the author has little faith in drugs. He summarizes it thus—nourishment, opium, alcohol, local applications. He has seen more harm than good from the administration of emetics to relieve the loaded bronchial tubes. The mortality of acute broncho-pneumonia is very high, and contrasts strongly with that of lobar pneumonia. The death-rate of the cases under treatment was 40 per cent.: of the cases in which the disease was secondary, 46 per cent., and of those presumably primary, 29 per cent. died.—*New York Medical Record*, 14th February 1885.

ON THE ACTION OF ANTIPYRIN UPON THE TEMPERATURE AND TISSUE CHANGES IN CHILDREN DURING FEVER AND IN HEALTH.

—After a *résumé* of the experience of others on the use of this drug, the author recounts his observations in eleven cases, viz.,—four of typhoid, one of tubercular meningitis, two of miliary tuberculosis, and four children in good health. The experiments appear to have been conducted with thoroughness and care. The dose given within twenty-four hours varied from 15 grains to a child of two years old, to 105 grains to a healthy child of seven years. The following were the results:—Antipyrin lowered the temperature in all cases, but to a very slight degree in the healthy children. The amount of fall in the temperature did not always depend on the dose. After even the largest doses, the temperature did not remain at its lowest figure more than twenty hours. The greatest fall occurred about midnight, after which the temperature gradually

rose. In healthy children, even large doses fail to lower the temperature to the same extent as in the feverish. Young children take large doses well for from one to two days. In most of the cases the quantity of the urine was diminished to the extent of nearly one-half; it was very concentrated, and the specific gravity was increased. The quantity of urea, uric acid, sodium chloride, phosphoric acid, and sulphuric acid was notably diminished during the use of antipyrin. Forty-eight hours after the last dose, the daily quantity of all these urinary constituents was greater than before the drug was given. The author feels some hesitation in recommending the use of antipyrin in fever cases; because, in spite of its undoubted antipyretic power, it could not safely be used for any length of time, if the remarkable retention of the products of oxydation which he observed is confirmed by further experiments.—Dr Jacobowitsch, *Jahrbuch für Kinderheilk.*, B. xxiii. Heft 4.

ON THE SURGICAL TREATMENT OF INFANTS.—It is of the greatest importance, says Dr Willard, to know thoroughly the anatomy of the child, as the position of organs, and especially the size of organs and canals, differs so widely from those of the adult, and mistakes are very easy. Ether should be used in any operation which gives much pain; antiseptics are essential. The author insists on early attention to congenital defects, and warns against the danger of delay. A rapid sketch is given of his treatment of the common surgical ailments of childhood, to some of which we call attention. Prolapsus ani, where it resists astringents, replacement, and similar measures, must be cured by the production of linear eschars by nitric acid. Epispadias and extroversion should be operated on as soon as the first dentition is complete. Adherent prepuce and phimosis can almost always be cured by separating the adhesions with a probe, which allows the prepuce to be drawn back; circumcision is very rarely necessary. Umbilical and inguinal hernia should be treated early, a truss being applied in the first few weeks of life. Most cases can then be cured. Harelip is operated on at the end of three months from birth; and instead of pins, a double set of sutures are used,—catgut for the mucous surfaces, and silk for the skin. Clubfoot should be treated immediately from birth, the foot being brought by manipulation into a position as near the normal as possible, and retained there by splints of felt, leather, or wood, rightly adapted. In the course of treatment the elastic strap is found very useful. In severe cases a subsequent tenotomy is necessary; but it is rendered more easy and effectual by the preliminary treatment. Failure after tenotomy is nearly always due to neglect of manipulation. In treating infantile paralysis, every effort should be directed to restoring power to the muscles and making them do work, apparatus only being used when the power is absolutely lost. Tracheotomy in croup is advocated, and the high operation preferred. Several other diseases are noticed.—*Philadelphia Medical Times*, 25th July 1885.

TWO CASES OF NEPHRITIS VARICELLOSA.—Nephritis is well known as a sequela of scarlatina, and occasionally of measles; but it is only within the last year or two that it has been observed to follow varicella, and very few cases have been as yet recorded. These two cases were observed at the Children's Hospital at Buda-Pesth by Dr Högyes. Case I., a boy, two and a half years of age, was admitted to the hospital for eczema of eyelids on 14th October 1884. On the 25th he had a mild attack of varicella, contracted from another child in hospital. On the 15th November his face was observed to be puffy, and his urine was found to be scanty, and to contain albumen. He had a very severe attack of nephritis, and died in three days. Post-mortem showed parenchymatous nephritis and œdema of lungs. Case II., a girl of seven years, came into hospital with varicella on 28th October; a moderately severe attack. On 21st November she was observed to have face and feet swollen, and the urine was found to contain albumen. She had a short but sharp attack of nephritis, but recovered, and was well by the 12th of December. She had suffered from no other disease, and the nephritis could only be referred to the varicella. The first symptoms of the kidney disorder appeared in each case from the nineteenth to twenty-first day after the first appearance of the exanthema.—*Jahrbuch der Kinderheilk.*, Band xxiii. Heft 3, 1805.

DIAGNOSIS, PROGNOSIS, AND TREATMENT OF RICKETS, by Dr Bohn, Königsberg.—Rickets, in the widest sense, is an extremely common condition, says this author; hardly 5 per cent. of all children escape it altogether. But we must not wait for our diagnosis and treatment until the bony skeleton shows it absolutely; it should be recognised and treated in its earliest stages. Diagnosis: Rickets depends on imperfect nutrition. Habitual constipation is often associated with its earlier development. Sometimes, on the other hand, there is diarrhoea. The earliest symptom is retarded or irregular appearance of the teeth, because the front teeth feel the want of ossifying material more than any of the other bony formations. The thinning of the bones of the skull, specially of the occipital bone, is another very early feature in the disease. *All thinning of the bones of the skull in the earlier months is due to rickets.* The edges of the sutures do not ossify as they should do by the end of the second month. These symptoms are often to be noted when the child appears to be otherwise in excellent health. The diagnosis between syphilitic and rachitic diseases of the bones in the earlier months can only be made in the living infant by the presence or absence of other signs of syphilitic disease. Prognosis and treatment must be considered together. The three essentials for spontaneous recovery are *suitable nourishment, fresh air, and sunlight.* The first of these requires the constant and minute attention of the physician. The general principles are the same, but the particular diet must be modified to suit individual cases.

The warm bath is of great value as an adjunct to the cure. The treatment by drugs can only be symptomatic, and directed against the various derangements as they arise. In rachitic anæmia, whether the child be fat or thin, iron is essential; and it is assisted in its action by the addition of a bitter. The value of cod-liver oil is recognised by all. In an appendix to the paper are some remarks on Kassowitz's treatment by phosphorus. It was introduced after the paper was written, and the author had not been able to make a full trial of it; but as far as he had gone, it had not come up to his expectations.—*Jahr. f. Kinderheilk.*, B. xxii. H. 3, 1884.

THE THERAPEUTICS OF HIGH TEMPERATURE IN CHILDREN.—For internal use the three principal antipyretics are aconite, quinine, and antipyrin. The indications for their use are thus summarized:—"Use aconite in small and frequently repeated doses when you have a hot dry skin with full and rapid pulse; use quinine when there is any suspicion that malaria is an exciting element; and use antipyrin when these have failed or are contra-indicated." Each of these remedies may be given by the mouth, by the rectum, or hypodermically. But temperature may be reduced easily and safely by the external application of water. This may be used in various ways: 1, tepid sponging, an easy and frequently efficacious method; 2, douche, only in desperate cases; 3, the bath, the use of which the author has abandoned; 4, cold pack, but this, to be of any service, has to be frequently changed, and constantly disturbs the patient; 5, cold enema, another remedy for desperate cases; 6, indirect applications, by means of the rubber coil applied to various organs, or the rubber cot. In all cases the *extremities are to be kept warm*, and that, too, by artificial heat.—W. Perry Watson, *Archives of Pædiatrics*, September 1885.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. ALLAN JAMIESON, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

ACUTE (ANGIONEUROTIC) CEDEMA.—Strübing, having had under observation some cases referable to the "acute circumscribed cutaneous œdema" of Quincke of Kiel, has made a careful study of them, and brought out some new features. Quincke had pointed out that in certain persons, after a chill or physical exertion, cedematous swellings of the skin and subcutaneous tissue were apt to occur over limited areas. The swollen parts were not sharply defined, the colour was seldom red, usually that of the normal skin, or pale and somewhat translucent. They appeared most frequently on the extremities, especially near joints, and at times in conjunction with exudation into these latter; but also on the trunk and face, and there particularly

on the lips and about the eyelids. They could further attack the mucous membrane of the throat and entrance to the larynx, and, causing constriction of the latter, might induce extreme dyspnœa. In one of Quincke's cases there appeared symptoms affecting the organs of digestion, as loss of appetite, vomiting, thirst, severe abdominal pain, and constipation, succeeded by diarrhœa when the attack had subsided. The œdema comes on suddenly, usually in several situations at once, reaches its maximum in from one to some hours, and lasts about a day. When retrogression takes place in one part similar swellings may develop on other and remote places. Thus the affection may drag on for days or weeks. The state of the general health varies—at times undisturbed, at others the patient feels out of sorts, with stupor, thirst, and diminution of the urine. The temperature during the attack continues normal. The œdema readily recurs when it has once shown itself, and commonly develops on the old spots. Men are more frequently affected than women, and like all neuroses it is hereditary. Details of three cases are given. A man, aged 70, who, since the age of 25, had had many attacks both of the cutaneous œdema and the gastro-intestinal symptoms—the latter were, in his case, the more frequent and the more prominent. All the features described above were present, and in addition there was a well-marked retraction of the abdomen during the attack, the epigastrium alone at the commencement of the seizure being slightly swollen. An only son, aged 16, suffered in precisely the same manner, and of three deceased daughters one had been attacked in the same way. The third case was that of a female. In her the vomiting and other gastric symptoms were most marked; the cutaneous were present, but subsidiary. Slight injuries seemed at times the starting-points of an outbreak, but the œdema transgressed the limits of the region injured. In considering the pathology of this complaint, Strübing draws attention to the œdemas which are met with in hydræmic states. In such we must assume an alteration in the vascular walls, in consequence of which the permeability is markedly increased. These changes in the function of the vascular walls can, in cases of acute œdema, only be produced by the influence of the nervous system. This must consist either in a failure on the part of the vaso-constrictors, or in over-activity or irritation of the vaso-dilators. An experiment of Ostroumoff's lends probability to the view that the essence of the acute œdema is to be sought in an increased irritability of the dilators. The gastro-intestinal symptoms in like manner can be partly but not wholly explained by the occurrence of an œdema of the mucous membrane, as the expression of vaso-motor disturbances. Leyden has shown that an array of symptoms, very like those described by Charcot as gastric crises in locomotor ataxy, and by other authors in various spinal diseases, may

occur independently of any disease of the spinal cord. The gastrointestinal symptoms in acute cedema closely resemble the "periodic vomiting" described by Leyden. Some of the symptoms, too, are like lead colic. Such are the retraction of the abdomen, the pain in the stomach, and the constipation. Riegel also has shown that in lead colic simultaneously with the accession of pain there is diminution of the urine. Observations on animals have shown that irritation of the splanchnic nerve causes contraction of the renal arteries, and a diminution of their lumen. The flow of blood to the glomerulus is thus impeded, and the quantity of urine is lessened. It is conceivable that both in the gastric symptoms of acute cedema and in lead colic this irritation may take place. The only treatment found available was that directed to modifying the symptoms.—*Zeitschrift für Klinische Medicin*, Oct. 1885.

LOCAL TREATMENT OF ERYSIPELAS.—Lewis, after recommending collodion in the early stages, relates his experience of white lead paint, if any considerable surface is affected, as suggested by Mr Richard Barwell. Lewis has employed no other local application, and has often used no internal treatment except the purge, which Mr Barwell advises to be administered. "Pure white lead paint of the shops is likely to dry too slowly, and I tell the painter to add some dryer as in ordinary painting, which in no way changes the effect of the application." The paint should be thicker than usual; it peels off readily when desquamation begins. While the remedy gives the patient a somewhat striking appearance at times, when a single ear or the nose is affected, yet it is a dry and cleanly dressing, very easily applied and rapidly successful.—*Journal of Cutaneous and Venereal Diseases*, September 1885.

BELLADONNA AS A MEANS OF PRODUCING TOLERATION OF IODIDE OF POTASSIUM.—From observation of the fact that belladonna produces dryness of the throat, mouth, and nose, Aubert got the idea of employing empirically this agent, in order to combat certain disagreeable effects of iodide of potassium. In three well-marked cases of naso-pharyngeal intolerance, the administration of the belladonna with the iodide gave good results. The same success was obtained in the case of a young man suffering from acute iodism, the symptoms disappearing by preceding the ingestion of the iodide with extract of belladonna. In this case the dose of belladonna was a pill containing two-thirds of a grain twice a day. In one of these cases, after the belladonna had been continued for several days, its use was intermitted, and the iodide, though still persevered with, was followed by no return of the intolerance.—*Journal de Médecine et de Chirurgie*, May 1885.

TUBERCULO-ULCERATIVE SYPHILIDE OF HEREDITARY ORIGIN.—Graham describes an interesting case of this where the first symptoms of syphilis appeared at the age of twelve. The girl's

father died when she was eighteen months old, suffering from tertiary ulceration of the throat. The girl did not present any of the features of hereditary syphilis. After a blow from a stick on the right forearm, which bruised the part, a local swelling formed, suppurated, and caused an ulcer. This slowly extended, an imperfect cicatrix resulting. It had in five years involved the skin from above the wrist to midway between the elbow and shoulder. There was also thickening and a callous sore over the left clavicle. The general health improved, and the ulceration healed under mild mercurial ointment locally, and perchloride and iodide internally. He raises the question not yet settled, Can such a lesion appear in a patient at the age of ten or twelve, or even later, who has not had infantile syphilis?—*Journal of Cutaneous and Venereal Diseases*, October 1885.

VARICOSE VEINS.—There are two theories as to the causation of varix. One of these places the origin with that of hæmorrhoidal tumours, as vascular growths; the other refers the formation of these venous dilatations to a hindrance to the return of venous blood. L. von Lesser has adduced fresh evidence of the plausibility of the first of these. He notes that in cases of varicose veins disturbances of circulation are not the prominent features, but disturbances of nutrition. Experiments have shown that an artificial thrombosis of the leg of a dog, produced by injecting a main venous trunk with gypsum under antiseptic precautions, does not cause varicosity of the veins. What was induced was a transitory congestive cedema, and a simple dilatation of collateral cutaneous veins. The dilatations of the veins of the neck in heart diseases are not varicose, but simple venous enlargements. For the production of true varices there must be a growth of the vascular walls, an increase in length and breadth. Further support to this view is afforded by the occurrence of varices in situations where mechanical impediments to the blood return are not present, as in varices of the lips, and within the mouth.—*Virchow's Archiv*, September 1885.

Part Fifth.

MEDICAL NEWS.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentlemen passed their final examination for the degree in Medicine at the sittings held on the 8th January 1886, and were admitted L.R.C.P. Ed.:—Samuel Hyde, Buxton; Robert Kidd, Tasmania.

ROYAL COLLEGES OF PHYSICIANS AND SURGEONS, EDINBURGH, AND FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.—The following gentlemen passed their final examination for the triple qualification in Medicine and Surgery at the sittings held

in January 1886, and were admitted L.R.C.P. & S. Ed., and L.F.F.P. & S. Glasg.:—William Edward Thompson, M.D., Ontario; Thomas James Barr, Calcutta; Jacob Lazar, Madras; Joseph Fayrer, London, W.; John Dickinson Leigh, Bishop Auckland; Charles James Morton, Edinburgh; Duncan M'Arthur, Clevedon, Somerset; Michael O'Sullivan, Bishop Auckland; Robert John Paton, Dumfries; Edward Thew Turnbull, Alnwick; James S. Sidey, Edinburgh; William Stewart, Edinburgh; John William Rowlands, Anglesey; Edgar Wakelam, Staffordshire.

FRIEDRICHSHALL MINERAL WATER.—This well-known mineral water has long been a favourite saline purgative, and is useful in many varieties of dyspepsia. For a sluggish liver and general torpidity of the alimentary tract it is one of the best of all the mineral waters. It contains in excellent proportions such saline purgatives as sulphate of magnesium and sulphate of sodium, combined with their chlorides, consequently its effect is at once mild, certain, and very efficacious in the treatment of congestion of the portal system and habitual constipation. The sulphate of sodium is an excellent hepatic stimulant, and is one of the principal salts contained in it. Having had numerous opportunities of witnessing its beneficial effects in the treatment of disease, we think it right to bring it under the notice of the profession as a valuable therapeutic agent.

OBITUARY.

DR THOMAS WILLIAMSON.

DR THOMAS WILLIAMSON has been suddenly and unexpectedly removed from the midst of his fellow-citizens, after about half a century of active service among them, as a general medical practitioner. On the afternoon of Tuesday the 29th of last month, while returning from a visit to a patient at Colinton, he was struck down with apoplexy upon the public road. Being at once recognised by a resident medical gentleman passing, he was promptly conveyed home, and the best skill at once secured, but he soon sank into a state of unconsciousness, and passed away very peacefully on Wednesday evening, in a little more than twenty-four hours after the seizure. He had reached his seventieth year, having been born at Greenock in 1815. After passing through the medical and surgical classes in the University of Edinburgh, he obtained the degree of Doctor of Medicine and the license of the Royal College of Surgeons while yet only twenty years of age. Almost immediately thereafter he entered upon the actual practice of his profession in Leith, and continued it with unwearied activity and devotion, and with considerable success, until his death. He was one of the oldest members of the Medico-Chirurgical Society and of the Royal Society, and in 1857 became a Fellow of the Royal College of Surgeons. For about thirty years he discharged the

duties of Surgeon to the Leith Hospital, and for several years was Physician to Gladstone's Hospital for Incurables. For a long period and up to his death, he held the office of Parochial Medical Officer and Vaccinator for the parish of South Leith, and was Medical Officer of Health for the burgh. The various important duties thus devolving upon him he performed with an ability, fidelity, and discretion, that secured for him the confidence and esteem of all those officially interested. He took a great interest in all his work, and even in the affairs of each of his patients. His discernment of character was remarkably correct, his judgment sound, and his amiability very marked, so that he was held in high estimation by all classes of his fellow-townsmen. He was an occasional contributor to the medical journals, published some pamphlets on Sanitation, and prepared some "Sanitary Notes" for the Social Science Congress held in Edinburgh, which were printed in the reports of that body. His work is now done, but he will be long remembered as a kind and honourable gentleman, and an esteemed and trusted physician.

DR DAVID GORDON.

By the death of Dr David Gordon another link to the past is severed, and another well-known southside practitioner is removed. Born in Canada more than 70 years ago, a graduate of 1841, for 45 years the tall, gaunt figure and grave, pleasant face of Dr Gordon have been familiar in Edinburgh, not only to successive generations of students who worked with him in the Dispensary, but to all those attending the scientific societies. For despite failing health, age, many anxieties, and depressing circumstances, few men were more zealous in the pursuit of practical knowledge, in constant self-improvement, than David Gordon was to the last. He was exceedingly kind, courteous, and painstaking in his work among the poor, an enthusiastic worker in midwifery, and one of the safest and boldest (safe perhaps because bold) chloroformists we have ever known.—*Requiescat in pace.*

JAMES GILCHRIST, M.D.

WITHIN a few months of the death of Dr W. A. F. Browne, his old friend and successor at the Crichton Institution has gone the way which we must all follow sooner or later. James Gilchrist studied at the University of Edinburgh, where he took the degree of M.D. in 1850. Shortly afterwards he became assistant to Dr Browne at the Crichton Institution, and in the autumn of 1853 was appointed Medical Superintendent of the Royal Asylum at Montrose, where he signalized his term of office by getting the Directors to

commence the new Asylum of Sunnyside. But before much progress had been made in this undertaking Dr Gilchrist was appointed Superintendent of the Crichton Institution on Dr Browne being chosen one of the Commissioners for Scotland in 1857. The Crichton Institution was a new and beautiful asylum erected for the wealthier classes, with a large building for the pauper lunatics of the Southern Counties within the same grounds. This important charge was held by Dr Gilchrist for twenty-two years, during which the Crichton Institution was distinguished by a remarkably low death-rate and a high rate of recovery. Neglecting nothing to qualify himself to be a true and wise physician for the insane, Dr Gilchrist lived strictly within the circle of his duties. Unassuming even to bashfulness, his life was spent in trying to soften the lot of the insane, to turn their thoughts into cheerful currents, and to hasten their recovery to reason. His finest qualities were more useful to others than to himself. To prefer realities to appearances is not a course likely to gain a man distinction in the charge of a Scottish asylum, and Dr Gilchrist may be said rather to have lived to do his duty than to seek credits and rewards to himself for so doing. Hence he never earned the full measure of reputation which he deserved, save from those who knew him well. I have often heard his former assistants speaking of Dr Gilchrist, and they always spoke very highly of him, of his kindness of heart, of his honourable and courteous disposition, of his watchful care of his patients, of his ceaseless efforts to keep them amused and happy, of his administrative skill, and his good sense.

In 1871, his old friend Dr Browne, now bereft of his eyesight, returned to Dumfries, and became psychological adviser to the Crichton Institution. Dr Gilchrist gladly accepted what aid his former chief could afford him to carry on his difficult and exhaustive duties. Of a nature both conscientious and sensitive, the worries and anxieties of asylum life in the end so affected Dr Gilchrist's health that he was obliged, in the autumn of 1880, to retire on a pension. He still continued to live at Dumfries, and spent much of his time in studying botany and natural history, giving lectures and instruction to a circle of friends and inquirers who gathered around him. In about two years Dr Gilchrist had to all appearance entirely recovered from the precordial distress and other symptoms of exhausted nerve function which had induced him to seek retirement. In a communication which he sent to the *Dietetic Reformer*, Dr Gilchrist attributed his recovery in great measure to a vegetarian diet.

Unhappily the hopes of his friends that his laborious life should be completed by a vigorous old age were not fulfilled. We learn from the *Montrose Review* that "he was able, in company with Mrs Gilchrist and niece, to attend the recent meetings of the British Association at Aberdeen, and to make a short visit to friends in Montrose on his return journey homeward. The revisiting of old

scenes and the renewal of old friendships afforded him genuine pleasure, and, it was hoped, would exert a beneficial influence on his health. But increasing weakness marked his course after his return to Dumfries; and ultimately an attack of congestion of the lungs brought to a close his long, and honoured, and useful life, as stated above, on Monday last," the 7th day of December 1885.

DR KELBURNE KING OF HULL.

By the sudden death of Dr Kelburne King, which occurred on Saturday 2nd January 1886, at the comparatively early age of 63, Hull has lost not only her chief surgeon, but one of her most prominent and eminent citizens.

Though he was for 35 years in practice in Hull, yet to many of the seniors among our readers he must still have been regarded as an Edinburgh man; for he was a distinguished Edinburgh student, President of the Royal Medical Society, Syme's house-surgeon and life-long friend, Knox's demonstrator; was an F.R.C.S. London and Edinburgh; an accomplished, brilliant, and daring surgeon; a most diligent citizen in all walks,—social, sanitary, and municipal. He was hospitable, cheery, friendly; in fact, in every way a noble specimen of a practitioner of the highest class, and he will be missed by many and mourned by all who knew him.

He died of sudden hæmorrhage into the trachea or bronchial tubes.

WILLIAM BOUSFIELD PAGE, F.R.C.S.,

Consulting Surgeon to the Cumberland Infirmary.

WILLIAM BOUSFIELD PAGE, who died on the 6th of January, was one of the ablest and best known of provincial surgeons. Born in 1817, and educated at the London Hospital, where he was an apprentice and a favourite pupil of the late Mr John Scott, he, at the instigation of his great master, came to Carlisle in 1842 as surgeon to the Cumberland Infirmary.

That institution had then but sprung into existence, and the position of its staff was therefore one of peculiar responsibility, for its well-being and its development lay largely in their hands. But the young surgeon proved equal to his position, for we find that within a very few years after his appointment, he could record in surgical annals success in ovariectomy, in excision of the knee, and in excision of the os calcis,—all operations which, at that time, were but struggling into recognition. That he thus rendered kindly aid in the establishment of them there can be little doubt.

The force and the faculty, which the performance, in those

earlier days, of such operations demonstrate that he possessed, led to his rapid success as a practitioner, and we find that within a comparatively short time, and while he was still a young man, he had become the confidential adviser of the best families in the county, and a recognised teacher in his profession, and this position he maintained for nearly forty years, carrying on a family practice of the most select class, acting as a consultant over the greater part of the north of England and far across the Scotch Border, always full of vigour, full of honour, and of wise counsel.

Added to all this he had soon one of the largest and widest connexions in the county as a railway surgeon. In this position the duties he undertook were of a more than ordinarily onerous character, and it was only by the wonderful combination of abilities he possessed that he was able to discharge them. In no one way could he demonstrate more forcibly and clearly the powers that were his, than in his examination of, and his report upon a railway case.

The perspicuity and the wisdom which enabled him to do this, associated as they were with great shrewdness and ripe experience, made him a solicited and much sought after consultant, and with many medical men in the north now there arises the feeling that a wise friend and a prudent counsellor has passed from them.

Educated at a time when the art of physical diagnosis was consequently in its infancy, and when the training of the student was very different from what it is to-day, he still managed to keep well abreast of the time; and to some of us it seemed on occasion little short of marvellous how, without the use of such means as we of necessity employ, he with that acumen which the very force of work had brought him, could make a more rapid and more accurate diagnosis. The regret and the sense of loss which by his death have come to the profession in Carlisle and its neighbourhood have found eloquent expression through Dr Hall and Dr Lockie, the presidents of the local medical societies of both of which Mr Page was a member.

A man of iron physique, of strongest mental capacity, full of mother energy, and of indomitable power of will, he yet possessed withal and so largely the better gifts of a tender heart, that directly through its influence rest came to him, for he died from one of those mysteriously progressive forms of anæmia, the starting point of which seemed to be in the shock of a heavy personal sorrow.

I have here shown a career of singular ability, importance, usefulness, and great personal influence, and the thought comes to me sadly how much of wisdom, how much of knowledge, and of rare tact have perished with him, for the position he held never appeared more truly unique than it does now, for it was one that no other man can well hold.—J. A. M.

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F. J. B. Quinlan, M.D., L.R.C.S.I., F.K.Q.C.P., M.R.I.A.,
etc., etc., *Professor of Materia Medica and Therapeutics, Catholic University Medical College; and Examiner in Materia Medica and Therapeutics, Royal University, writes in the "Medical Press," July 15th, 1885.*

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Part First.

ORIGINAL COMMUNICATIONS.

CONTRIBUTIONS TO PRACTICAL SURGERY.

By JOHN CHENE, Professor of Surgery, University of Edinburgh.

II. THE ABDOMEN AND PELVIS.

22. *Hernia*.—The application of a truss in the treatment of this common affection must not be looked upon as simply palliative. In old people with long-standing herniæ the curative action of a truss cannot be looked for; but in all recent cases at all ages a truss must be applied, not simply as a palliative, but in order to effect a cure. The younger the patient, the greater is the probability of this good result. The hernial protrusion, after it has once been reduced, should never be allowed to come down again. Although in the recumbent posture the chances of the hernial protrusion occurring are diminished, still, as any exertion, as in the act of coughing, may during the night cause the protrusion, a truss should therefore be worn day and night. During the night less pressure is required to keep up the hernia, and if a spring truss is irksome to the patient the hernia may be kept up by a thick pad of layers of lint or layers of flannel, fixed in position by an elastic spica bandage.

(a.) In inguinal hernia a double spiral truss is preferable to a single-headed truss. In young children the presence of an inguinal hernia on one side indicates a tendency to hernia on the opposite side; in the adult the same factor is at work, although in a less degree; for this reason the use of the double-headed truss is indicated. A double-headed truss also fits more comfortably, and gives that general support to the lower part of the abdominal wall which renders the patient infinitely more comfortable than if he used simply a single-headed truss. In inguinal hernia care must be taken that the pad of the truss does not press on the spine of the pubis. In the oblique variety the principal pressure should be over the situation of the internal abdominal ring, and the head of the truss should not extend to a lower level than immediately below Poupart's ligament. Otherwise, when the patient stoops, the tissues of the thigh, pressing on the lower border of the head

of the truss, are apt to displace it in an upward direction and render it inefficient, the hernia escaping below it. Although in some rare cases a perineal band may be necessary, every endeavour should be made to avoid its use, as it is irksome. The wearing of a piece of boracic lint below the pad of the truss prevents chafing and irritation of the skin, and the parts are kept dry, the presence of the lint allowing of free evaporation. The pad must be flat and have no tendency to press into the inguinal canal. All that is required is to support the weakened wall, and any pressure into the canal tends to weaken by atrophy the structures which form its walls, and in this way to prevent a radical cure. Everything should be done by the surgeon to imitate Nature's way of curing a hernia, namely, by contraction of the neck of the sac and contraction of the fascial structures which surround the neck of the sac. The surgeon should see to the application of the truss himself, and not trust it to any instrument-maker. The weaker the truss the better, as long as it fulfils its object, namely, keeping the hernia up. Each time the hernia is allowed to come down the tissues are stretched. The good work of weeks is undone by a single protrusion, and hence the importance of explaining to the patient the necessity of never allowing the hernia to come down.

(b.) In umbilical hernia the natural tendency to contraction of the abdominal opening is interfered with if a nipple-shaped pad is used. The pad must be flat, of a considerable size and thickness, and in the child it is best kept in position by the use of a broad elastic bandage. If the pad is made of layers of flannel, it should be placed next the skin, under the chemise; the elastic roller is then passed round the body over the chemise, and a large safety-pin is passed through the bandage, chemise, and pad, fixing the last firmly in position.

(c.) In femoral hernia the great depth of the crural ring through which the hernia protrudes prevents pressure being made directly upon it, and in this form of hernia the use of a small nipple-shaped triangular pad, fitted on to the ball and socket joint of the Salmon & Ody truss, as first recommended by Professor Spence, is the most efficient means of keeping up the hernia. By the judicious use of what is termed a keeper, the lower extremity of the pad can be tilted upwards and backwards, increasing the efficiency of the appliance. In some cases of inguinal hernia, instead of the fixed head, the Salmon & Ody ball and socket pad arranged with a keeper may be found useful. In the *British Medical Journal* for 10th October 1885, Mr Rawlins suggests an arrangement in which a double-headed Salmon & Ody truss may be applied; this method appears in principle to the writer to possess many advantages, and it should certainly have a fair trial.

(d.) A hernia, apart from strangulation, is irreducible either in consequence of adhesions of the omental contents to the inner surface of the sac wall, or in consequence of induration and chronic

enlargement of the omental contents after they have passed into the hernial tumour. In the former case, in order to reduce the hernia, a cutting operation will be necessary, opening the sac, dividing the adhesions, and returning the contents. In the latter case, namely, where there is induration with thickening of the omental contents, the reduction of the hernia is generally effected by prolonged treatment in the recumbent posture, the foot of the bed being elevated, and the patient kept on low diet. Some years ago the writer had under his care a patient with a large irreducible inguinal hernia, in whom repeated attempts had been made to reduce the hernia by the methods indicated above. In that case, the continuous use of elastic pressure over the whole surface of the inguinal tumour, by means of an elastic bandage, enabled him to completely reduce the hernia after three days of continuous pressure. Since that time several cases of irreducible inguinal, femoral, and umbilical herniæ have been treated in this way with most satisfactory results; and in every case of irreducible hernia this method should receive a fair trial before cutting down and relieving the irreducibility by operation.

In the reduction of an inguinal hernia the mobility of the neck of the sac, where the constriction exists, is a great obstacle to reduction. It is therefore necessary to fix the neck of the sac with one hand before applying pressure over the hernial tumour with the other hand. If the hernia is of large size, the reduction is often facilitated by getting an assistant to fix the neck of the tumour, the surgeon applying pressure with both hands over the hernial mass. In femoral hernia it is impossible to fix the neck, and, although the anatomical relations here render the neck of the sac less mobile than in inguinal hernia, still the impossibility of grasping the neck during taxis is in all probability the main reason why taxis fails more frequently in femoral than in inguinal hernia. The pressure in femoral hernia is mainly on the apex of the tumour, and as the effect of pressure is often simply to change the shape of the tumour, its reduction is not effected. In inguinal hernia the neck of the sac can be fixed, and the pressure can be applied over the surface of the tumour in a more efficient manner.

(c.) When a hernia is strangulated, that is to say, when there is an obstruction to the blood-flow into and out of the hernial tumour, if taxis fails, there should be no delay in operating. It is often a very difficult matter in practice to distinguish a strangulated hernia from an inflamed hernia, in which there is an inflammation of the contents of the hernial tumour apart from strangulation. It is also difficult to diagnose a strangulated hernia from an obstructed hernia, in which there is an obstruction to the free passage of the contents of the bowel within the hernial sac. In the first case it is a local peritonitis, in the second a local constipation. Both conditions, if they persist, end in strangulation. When the surgeon is in any doubt, he should operate. In all cases in which

there are any symptoms which may be referable to a strangulated hernia, a careful examination by sight and touch must be made of all the situations in which a hernia may exist; and if in any of these situations a swelling is discovered, however unlike it may be to a hernial tumour, the surgeon should cut down upon the swelling, and carefully examine its exact nature. He may find, for example, only an inflamed gland, or he may find a small hernial tumour behind an inflamed gland. In the former case no harm has been done; in the latter case delay would probably mean a fatal result. Do not be deceived by a free passage of the bowels after the symptoms of strangulation have commenced. In women more particularly, two or even three free movements of the bowels may take place after complete strangulation. The bowels being loaded with fecal matter, the passage of this fecal matter per anum deceives the surgeon, and he delays operating until it is often too late. After operating, relieving the constriction, and returning the contents, ligature the neck of the sac, and stitch together the structures which surround the neck—perform, in other words, a double operation: firstly, relieve the strangulation; and, secondly, attempt to cure permanently the tendency to the formation of a hernia. In performing the radical cure, therefore, either after the operation for strangulated hernia, or after operating on an irreducible hernia which has resisted carefully applied elastic pressure, or after operating on a reducible hernia which cannot be kept up by a truss, imitate Nature's method of cure,—ligature with catgut the neck of the sac, and stitch together the anatomical structures which surround the neck of the sac.

23. *The Rectum*.—In all rectal affections there is often great depression of spirits. There is a close physiological connexion between the genito-urinary tract and the rectum. Very frequently a patient will come to you complaining of irritability of the bladder, which may be due to some rectal irritation, e.g. piles or fissure; and it is, therefore, a good rule in all cases of irritable bladder to make a careful examination of the rectum, anus, and perinæum. In all rectal examinations care must be taken to ascertain before making the examination that the patient is not suffering from acute syphilis. The discharge from an ulcer when the patient is in this state is infectious, and if there is any abrasion on the finger of the surgeon, he may be inoculated during the examination. In an examination of the prostate it is best to place the patient on his back; but in an examination of the posterior rectal wall the patient is best lying on his left side. In the married female, the surgeon with two fingers introduced into the vagina can by pressure cause protrusion of the mucous membrane of the rectum, and in this way assist the diagnosis in cases of fissure and of internal piles. The best way, however, of examining a male patient is to make him lean over the back of a chair. If the buttocks are then separated, much may be learned by a visual

inspection of the perinæum. If there is any discharge the skin of the perinæum will be more or less excoriated, the area of excoriation corresponding to the portions of skin in apposition with each other. If there is a fissure, the opening of the anus will be firmly closed by the contraction of the sphincter, and the patient shrinks from any tactile examination. One small external pile is often an indication of the presence of a fissure at its base. A venous pile, due to the rupture of a small vessel at the junction of the skin and mucous membrane, will be indicated by a bluish coloured projection at the seat of the rupture. A redundancy of the folds of skin around the anus indicates in all probability the presence of internal piles within the opening. A leathery appearance of the skin around the anus is seen in the affection termed *pruritus ani*. After a careful visual examination the finger well oiled is to be introduced gently, and with a rotatory movement into the cavity of the rectum. In some rectal conditions associated with an abnormal dryness of the mucous membrane in the narrow part of the canal corresponding to the internal sphincter, this dryness will be best overcome by the injection of an ounce of warm oil before attempting to introduce the finger. After the finger is fairly within the rectum, inflammatory affections of the mucous membrane will be indicated by increased heat within the rectum. The presence of a fissure will be discovered as the finger is being introduced. Some fissures lie immediately within the opening. In this case the lower extremity of the fissure will be observed by forcibly separating the buttocks, and stretching the skin around the anus. In other cases the fissure lies altogether within the opening, and can only be diagnosed by feeling it with the finger. An anal speculum may be used in doubtful cases, but the educated finger is the best diagnostic. The condition of the prostate is carefully to be noted. (1.) With regard to its size and its shape, increase in size of one or other of the lateral lobes indicating lateral hypertrophy of the gland. (2.) As to the presence of a soft point or points, indicating either inflammatory abscess or tubercular deposit in the process of disintegration.

In the examination of a fistula with a probe, the finger in the rectum will enable the surgeon to discover more easily the internal opening. In stricture of the rectum the stricture is almost always within reach of the finger, and the condition of the stricture can be carefully examined. After the finger is removed the presence of pus or slimy blood-stained mucus will indicate the existence of an ulcerated surface. *Diarrhœa* is often a symptom of stricture of the rectum, and in anyone above middle age who suffers from persistent *diarrhœa* a rectal examination should always be insisted upon in order to assist the diagnosis.

24. *Hæmorrhoids*.—The presence of internal hæmorrhoids is local evidence of a general congestion of the portal system, and

only when there is great local discomfort, and after every endeavour has been made to improve the general condition of the patient by medicinal treatment, should operative procedure be undertaken. There is great difference of opinion as to the best method of treating internal piles when operation is necessary. If the ligature is used it should be soaked in a solution of chloride of zinc, 40 grs. to the ounce. It must be tied tightly, completely strangulating the pile. If the ligature is tied loosely, acute inflammation followed by gangrene is the result. During the inflammatory stage, pain is severe, and many of the evil consequences which have followed the ligature have been due to its imperfect use. The injection of 10 drops of a 4 per cent solution of cocaine into the base of the pile before applying the ligature greatly relieves the pain. If the pile be pedunculated, its base may be constricted by simply passing the ligature round it and tying it tightly. If the pile is sessile, its base should be transfixed with a curved needle carrying a double thread, each ligature being tightly tied so as to constrict one-half of the base. If the base of the pile is close to the opening of the anus, the division of the mucous membrane at the verge of the anus with scissors before tying the pile assists the more effectual application of the ligature and greatly lessens after-discomfort. If there is more than one pile, each pile must be attacked separately in this way. The ligatures being cut short, the strangulated masses are then pushed back, and a $\frac{1}{2}$ grain morphia suppository introduced into the cavity of the rectum. The patient's bowels should have been thoroughly emptied before operation. The diet should be very light after the operation, and it is not necessary that the patient's bowels be moved until the third or fourth day. This is best done by the administration of castor oil by the mouth, and the discomfort felt during the movement of the bowels is greatly alleviated by the injection of 4 oz. of warm olive oil. Many patients suffer from retention of urine after this and other operations on the rectum, and it is therefore necessary to see the patient on the evening of the operation, and if he has not made water, a red rubber instrument must be introduced to evacuate the contents of the bladder. The use of the clamp and cautery has to a certain extent displaced the use of the ligature, but in the opinion of the writer the ligature, if properly applied, is as efficient as the cautery, and is less likely to be followed by hæmorrhage. Never operate in a case of internal piles without having first satisfied yourself that the patient is not suffering from cancer of the rectum. External piles are to be treated by removal with a pair of curved scissors, the cut surface being touched with a solution of chloride of zinc, 40 grs. to the ounce.

A patient may consult you, complaining of having felt a sharp, cutting pain during the passage of a hard fæcal mass. Visual examination of the anus will at once indicate what has happened. A small tumour of a bluish colour is seen at the opening of the anus, where the skin and mucous membrane meet. A sub-

mucous vessel has given way, and the extravasated blood clots. This condition may be treated by fomentations and rest. After a time the clot will be absorbed, but the process is a slow one, and it is better to transfix the mass with a sharp-pointed, curved bistoury—the clot being squeezed out and the cavity touched with a solution of chloride of zinc, 40 grs. to the ounce.

25. *Fissure*.—The pain in fissure is of a twofold character: a sharp, cutting pain felt during movement of the bowels, and a throbbing, persistent uneasiness coming on after the bowels are moved, and lasting for a variable period. The primary pain is due to the stretching of the part. The persistent after-pain is due to spasmodic contractions of the sphincter. The base of the fissure consists of muscular tissue, and free division of this muscular base relieves the symptoms, gives the part rest, and allows the ulcer to heal. The injection of cocaine into the tissue forming the base of the ulcer renders the operation painless. The division of the base of the fissure, however, does not always give relief, and in a severe case of fissure it is more satisfactory to place the patient deeply under the influence of chloroform, to introduce the thumbs within the anus and forcibly to stretch the sphincter, dividing at the same time the base of the fissure. Introduce a morphia suppository after the operation, and prevent movement of the bowels for three or four days.

Division of the base of the fissure is very frequently done by introducing the finger into the anus, and then, with the finger as a guide to the upper extremity of the fissure, a sharp pointed curved bistoury is introduced at the side of the anus and carried through the base of the fissure up to the point of the finger. This method of performing the operation is dangerous, and surgeons have been attacked with syphilis in consequence of pricking the finger with the point of the bistoury. The safer plan is to lay a straight blunt pointed bistoury flat upon the finger, to introduce the finger with the bistoury in position, to feel for the upper extremity of the fissure, and then to turn the sharp edge of the bistoury towards the ulcer, the back of the bistoury lying on the finger. The knife is then carried through the base of the fissure by the pressure of the finger. The bistoury to be used, though probe pointed, must have a cutting edge to its extremity.

26. *Prolapsus Recti* in the child is very often a symptom of stone in the bladder. The child may be brought to you with the prolapse down for some time. It may be reduced by manual pressure, but the simplest way to reduce it is by elastic pressure with a T shaped bandage, placing a pad of cotton wool between the bandage and the prolapse. In elderly people, prolapse of the rectum is generally due to chronic relaxation of the sphincter ani, and levator ani muscles. The removal of the redundant skin around the dilated anus may assist in keeping up the prolapse. Special instruments are made for preventing its descent, but the most efficacious method is a double T bandage, with two vertical limbs, each

one attached, both in front and behind, to a firm pelvic band, and crossing in the perinaeum over the anus. At the point where they cross, a pad of lint is interposed between the bandage and the anus.

27. *Fistula in Ano*.—The most satisfactory way to divide a complete fistula is to pass a probe from the external opening along the sinus, through the internal opening, and then by bending the probe to bring its point out at the anus. The probe is then cut out, and the septum is in this way completely divided. There is a groove in the probe which acts as a guide to the knife. Care must be taken in the after treatment by stuffing the wound, to cause it to heal from the deepest parts towards the surface.

URINARY ORGANS.

28. *Bladder*.—Chronic cystitis is one of the most troublesome affections that the surgeon is called upon to treat. The primary cause is often due to the introduction of organisms with an unclean instrument, and therefore in all cases in which, by the presence of increased quantities of mucus or of pus in the urine, along with frequency of micturition, a diagnosis of cystitis is made, the urine should be carefully examined for the presence of organisms. If they are present, corrosive sublimate should be administered internally in small doses, and the bladder should be washed out with antiseptic solutions. No rule can be laid down as to the special antiseptic to be used. The great point to attend to is that the bladder be repeatedly filled and emptied until the fluid escapes free from all sediment. The best way to wash out the bladder is to introduce a red rubber catheter with a "velvet eye," and to attach to this a T tube of glass. To the vertical branch of the T an indiarubber tube four feet long is fixed, and is passed into a vessel containing the fluid to be injected. This fluid should be tepid and contain an antiseptic. To the horizontal limb of the T another piece of tubing is attached, in order to carry the fluid from the bladder. The outlet tube being pressed with the finger and thumb, the vessel connected with the inlet tube is raised, and the fluid is allowed to flow into the bladder until the patient feels slight discomfort. The inlet tube is now grasped with the finger and thumb, and the fluid is allowed to escape by the outlet tube. This is repeated again and again until the bladder is thoroughly cleansed. In many intractable cases this method of treatment is not sufficient, because the bladder is an organ which is never at rest. When it is inflamed from any cause, its diastole and systole, instead of being repeated three or four times within the twenty-four hours, takes place much more frequently. In such cases it will be necessary to give the bladder rest, either by bladder drainage, according to the method recommended in the *Edinburgh Medical Journal* for December 1880; or should this plan fail, as it may do if there is much mucus which plugs the tube, the membranous urethra must then be opened on a grooved staff, and a

tube introduced into the bladder through the wound, which allows the urine constantly to drain away as it escapes from the ureters. The tube introduced should be as large as possible to prevent any risk of blocking, and the bladder should be washed out through this tube, so as to clear away any mucus which may collect at the floor of the bladder. Such a tube may be kept in for a fortnight without risk of a persistent fistula remaining.

29. *Hypertrophy of the Prostate*.—If a patient says he has to rise at night to make water, and that he makes water during the day with increased frequency, always be suspicious that he is not emptying his bladder completely. He makes only the overflow from a distended viscus. When there is a tendency to obstruction of the flow of the urine in consequence of hypertrophy of the prostate, there is little doubt that the occasional passage of a large-sized bougie is of value in keeping the channel patent.

30. *Stricture of the Urethra*.—Organic stricture of the urethra is caused either by an injury to the urethra or by gonorrhœa. After a gonorrhœa passes into a persistent gleet, the source of the discharge is generally from the urethra, in the neighbourhood of the triangular ligament. This is the common situation of stricture. The patient may be brought under your notice for the first time suffering from retention of urine, or he may come complaining of a decrease in the size of the stream. The contraction of the urethra is so very gradual, that the stream may become of very small size before it attracts the notice of the patient. No method of relieving stricture is permanent, and after the urethra is restored to its normal calibre, the patient must always be informed that the occasional passage of an instrument during the remainder of his life is the only certain way of preventing a recurrence of the condition. In uncomplicated cases of stricture the treatment by gradual dilatation is the surest and safest method. In anterior strictures internal division may be adopted; in posterior strictures at the triangular ligament, if the stricture is of a resilient character, with great tendency to rapid recurrence, Holt's method may be used. If there is much perineal induration, more especially if it is complicated with perineal fistulæ, external division is the best remedy. These three latter ways of treating stricture are only to be used in exceptional cases. The great majority of cases of stricture should be treated by gradual dilatation. This can be done in two distinct ways. If the patient can keep his bed, a gum elastic catheter is passed through the stricture into the bladder. The size of the instrument will depend upon the size of the stricture. If, for example, a No. 2 is passed, and is not tightly grasped at the strictured part, a No. 3 is passed. This may also pass easily, and not be grasped. If so, a larger instrument must be introduced. If, however, it is grasped, it is fixed to the penis with a piece of sticking plaster. During the first few hours the urine flows through the instrument, but on the following day, when

the patient makes water, the urine escapes along the outside of the instrument. The instrument is now loose. By its pressure it has caused absorption of the inflammatory lymph or fibrous tissue under the mucous membrane. A larger sized instrument is then passed. It in its turn is grasped, and gradually, the process being repeated, a No. 12 is at last reached. The rapidity with which this point is arrived at will vary, but, as a rule, in uncomplicated cases it is generally found that each day represents a number in the size of the instruments, and in from ten days to a fortnight the urethra is dilated to its normal calibre. It is well not to stop at No. 12, but to go on to No. 14. This method of treatment requires constant confinement to bed.

The other method of treatment is intermittent. The stricture is gauged with a bougie. Let us suppose that a No. 3 can be passed. The result of the passage of this instrument will probably be slight decrease in the size of the stream on the following day. On the third day there will be improvement; on the fourth day a No. 3 can be passed with ease. A No. 4 is then passed, and perhaps even a No. 5 can be passed. The great point to attend to is not to attempt too much at each time, but to be satisfied with the passage of one or two instruments of a larger size than the one that was previously passed. Gradually by this method the full size is reached. If the patient can keep his bed during the cure, the instruments may be passed every third or fourth day. If the patient is going about, an interval of a week between the passage of the instruments is preferable. There is much difference of opinion as to the best form of bougie to use. Many surgeons use the French bulbous-pointed flexible bougies. Other surgeons use a metallic instrument—either the cylindrical bougie with a rounded point and of equal calibre throughout, or a solid metallic instrument with a bulbous point and gradually increasing from the neck, so as to be more or less of a wedge shape. The writer prefers the metallic instruments, and if the bulbous-pointed metallic instruments are used, care must be taken not to exert any force in passing the bougie, because its wedge shape, especially as this is very gradual, greatly increases its power, and, in fact, it may be used forcibly to distend the stricture. If used in this way, it does not act in the same way as the old cylindrical bougie, which is intended simply to set up a slight irritation, and cause absorption of the fibrous tissue. If the wedge-shaped bougie is used, it may not only do this, but may mechanically stretch the strictured part, and to a certain extent approach in principle to the other mechanical means used, such as Holting, internal or external division, in all of which the action is mainly mechanical, the fibrous tissue being ruptured or divided.

31. *Retention of Urine.*—(a.) Retention may occur after an operation, such as ligature of internal piles. In this case the condition is a purely reflex one, and is easily relieved by the use of a

large-sized red rubber instrument, thoroughly purified by injecting through it a weak solution of corrosive sublimate, and anointed with a weak antiseptic oil, *e.g.*, vaseline mixed with oil of eucalyptus, half a drachm of the latter to an ounce of the former.

(b.) Retention of urine may also occur as a complication in an acute gonorrhoea. This is rare if the urethra has previously been perfectly healthy. In such cases there is a combination of acute inflammation with spasm, and every endeavour ought to be made to relieve the condition by the use of hip-baths, hot fomentations, large doses of bromide of potassium, and morphia suppositories. An instrument should only be used when these fail. A flexible instrument will not do; and a metallic instrument, which should be of large size, is necessary. The catheter should be thoroughly purified by passing through it a stream of 1 to 20 carbolic lotion; and the form of metallic catheter most easily kept pure is that in which the portion between the eye and the point of the instrument is solid, instead of forming a little cul-de-sac, which it is often very difficult to cleanse thoroughly. The spasm is overcome by gentle steady pressure. Even with the greatest gentleness there is often severe pain, and the injection of a drachm of a 4 per cent. solution of cocaine before introducing the instrument gives great relief.

(c.) Retention of urine may also occur in consequence of the superaddition of an acute inflammation, with spasm, to an old standing organic stricture. Here, again, the hip-bath, with the other means above recommended, should always be tried in the first instance before attempting to pass instruments; and if the surgeon has no previous knowledge of the case, and is unacquainted with the size of the organic stricture, he should not at once use a small instrument, but begin with a No. 6 or No. 7; and if he fails to pass this, he may then try smaller instruments. The smaller the instrument the greater is the risk of laceration of the mucous membrane of the urethra. If, after a fair trial with instruments, he fails to relieve the retention, he should aspirate above the pubis, continue the use of fomentations and sedatives, and on the following day he will find either that the retention is relieved, or that he is now able to pass an instrument along the urethra into the bladder. This use of aspiration is of value. Repeated aspirations in bad cases of stricture with retention are not, however, to be recommended. In such cases there is a tight stricture, and it is best here to pass a large-sized instrument down to the stricture, and the patient being tied up in the lithotomy position, to cut down in the middle line on the point of the instrument, to open the urethra, and, using a fine grooved probe, to search for the stricture; and, passing the probe along it, to divide the stricture with a narrow knife, passed along the groove in the probe. A full-sized gum elastic catheter is then tied into the bladder. In this way the retention is relieved, and the stricture is divided.

The tolerance of instrumental interference with the urethra

varies very greatly in different people, and it should be a rule in practice, in cases in which the surgeon is entirely ignorant of the sensitiveness of the patient, not to pass an instrument, for the first time, in any circumstances in which the patient may be exposed to wet or cold. Before the passage of an instrument, it is well to administer 5 grains of quinine, or some of the more recently introduced antipyretics, *e.g.*, kairin or antipyrin. These remedies have an undoubted value in checking urethral fever. Their power is increased by giving the patient a drink of warm gruel immediately after the instrument has been passed. Shivering and rapid rise in temperature, after the passage of a bougie, must not be confounded with the so-called "catheter fever," which has within recent years been brought prominently under notice by Sir Andrew Clark. The "urethral fever" is a reflex condition, depending upon the sensitiveness of the urethra. The true cause of "catheter fever" was fully explained at the discussion on Sir A. Clark's paper, read before the Medico-Chirurgical Society of Edinburgh (see *Edinburgh Medical Journal*, April 1884).

(d.) Retention of urine in old men is generally due to a congestive attack of the prostate superadded to hypertrophy of the gland. Here, again, the congestion should, if possible, be relieved by hip-baths, fomentations, and sedatives, and, if instrumental assistance is required, in the great majority of cases the red rubber instrument relieves the retention. If the instrument fails, then a metallic instrument is necessary. In cases in which there is a distinct valvular obstruction from enlargement of the middle lobe of the prostate, the difficulty may be overcome by passing a large-sized gum elastic catheter with a metallic stylet *in situ* down to the obstruction. If the instrument is then withdrawn to the extent of an inch, by pulling on the stylet, the point of the catheter will rise vertically in the bladder.

(e.) Retention in young children is very frequently due to the presence of a calculus in the urethra. In rare cases it may be due to malignant disease of the prostate, and sometimes it is due to abscess in the prostate. These conditions are comparatively rare, and need not here be dwelt upon.

II.—DISEASES AND REMOVAL OF THE UTERINE APPENDAGES. ABSTRACT OF A LECTURE DELIVERED BY MR LAWSON TAIT TO THE MEMBERS OF THE CLASS OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH, ON 18TH DECEMBER 1885, AND REPORTED BY J. MACINTOSH CAMPBELL, M.A.

MR LAWSON TAIT prefaced his remarks by declaring that he was quite unaccustomed to public lecturing, and certainly to teaching—in fact, in his part of the world teaching did not pay.

They must not therefore expect the finished production of one skilled in the art of teaching, such as exist in large numbers now in Edinburgh. What he had to talk about was the experience of a mere practitioner.

In drawing attention to the removal of the uterine appendages, Mr Tait remarked that all our old terror of the abdomen had vanished, and now scarcely a day passed in which he did not open an abdomen for some cause or another, and frequently he was called upon to open several in one day. He considered that while the pathology of the ovaries was pretty well exhausted, a great deal remained to be done in connexion with their physiology. Until quite recently it was commonly believed that menstruation depended upon ovulation. Now, however, we know that while menstruation occurs only at a definite and regular time, ovulation may take place at any period, and it certainly was by no means so frequent as menstruation. To-day he meant to confine his attention to a consideration of the Fallopian tubes, and in the first place to

Ordinary Inflammation.—The history that one gets in such cases may be of several kinds:—(a.) We not unfrequently find salpingitis in unmarried women. Probably we will be able to trace the history of a connexion and a consequent gonorrhœa. Many of our surgeons were inclined to consider gonorrhœa in the male a very trivial and a very unimportant disease. To such an opinion he took very strong exception, because he considered that a very great proportion of the special diseases of women were directly or indirectly attributable to the effects of a latent gonorrhœa in the male. In continuing our inquiry we would find the existence of pain in the pelvis, and discover that the onset of this pelvic pain had been sudden in its character. (b.) In other cases we would get quite a different history altogether. The patient is a married woman. She has had, perhaps, one child, but has never been well since her delivery, and has never been again pregnant. Or in some cases we get the history of a miscarriage two or three months after marriage. The patient has never been well since her miscarriage, nor has she been again pregnant. Inquire a little carefully into these so-called miscarriages, as not unfrequently they are merely local peritonitis. (c.) Not only have we these symptoms following a gonorrhœa, a bad labour, or a miscarriage, but we have them equally marked in cases where the causes are altogether unaccountable. This, however, is by no means very common. Sometimes we find them in young girls between the ages of sixteen and twenty, and who are pure virgins. It goes under the name of "catarrhal salpingitis," and we frequently can trace the commencement of the symptoms to a chill after a dance, or to sitting on damp grass after playing tennis. In studying the symptoms of a case we must determine one or any of these causes. In acute cases we find the history of rigors, pelvic uneasiness, pain,

and swelling. On opening the tube we note the redness, marked congestion and inflammatory effusion, while the inside of it is bathed with pus.

Chronic cases are, however, by far the commonest. In these we find that menstruation is usually very frequent, coming on every sixteenth day or so, and extending from five to ten days. The discharge is profuse, and dysmenorrhœa is marked. We should always be especially suspicious of salpingeal disturbance if the pain comes on a day or two previous to the menstrual flow. It is due to some rhythmical expulsive contraction in the inflamed tubes. This occurs normally before each menstruation, and accounts for the old idea of ovulation occurring at each menstruation. There is marked pain on movement of any kind—either walking or even driving in a carriage—while standing any time causes aching in the back.

On examination the uterus is nearly always found to be fixed. The ovaries are generally small, and are also bound down by adhesions. The Fallopian tubes are found adhering to the pelvis, and are frequently occluded. The patient is thus completely sterilized by the disease itself, and this fact removes the objection that surgical interference prevents any further impregnation. Besides, the operation relieves the great pain and suffering. The dyspareunia is removed, and thus operative procedure is the only possible way of resexing the patient. Before discussing the treatment Mr Tait went on to consider the subject of

Fallopian Pregnancy.—The uterus, he said, was the normal place of impregnation. When, however, the epithelium of the Fallopian tube is destroyed by any cause, we occasionally find spermatozoa going up the tube because they have no cilia to fight against. Under such circumstances we meet with Fallopian pregnancy. In such cases the bursting of the Fallopian sac is a very frequent cause of a fatal collapse. We find the tube can be distended up to a certain length, the limit being to a diameter of about three inches. This is reached about the tenth or twelfth week of pregnancy. The chief symptom in tubal rupture is the hæmorrhage, and the amount of this we find depends upon the placental insertion. Where it is attached into the upper part of the tube, the rent occurs there, and the hæmorrhage is very fatal, and takes place into the pelvic cavity; when it is inserted low down, the discharge takes place into the folds of the broad ligament, and is comparatively small in amount. It is in the latter case that we find the child frequently going on to the full time.

Occlusion of the Tubes.—As the result of tubal irritation we sometimes find the fimbrial extremities occluded. The uterine end is also frequently closed. If not, we have periodic discharges, sometimes of a serous character, often of pus. In very chronic cases we may have caseation, and this may set up tubercular diseases in neighbouring or distal organs. The patients find coitus impossible from the intense pain.

As regards *treatment*, in acute salpingitis we should operate *at once*. Many patients die in the acute stage. Besides, the suffering is intense and prolonged, while the prognosis is always unfavourable. In chronic cases also operate where the history helps you to diagnose with any degree of certainty, and especially where physical signs confirm your opinion. If not, be far more guarded; because if salpingitis is not the real cause of the pain, although the operation may relieve the patient for a time, it is almost sure to return. It is in such cases that we get unsatisfactory results from operation, and, therefore, it is that what is known as Battey's operation has led to so much disappointment. What Battey desired was to cure various and vague reflex symptoms by a premature climacteric. Mr Tait had followed Battey's example in a few cases, but was not encouraged by the results.

Abdominal Section.—The incision must always be very small. Many medical men, who had been present at some of Mr Tait's operations, had complained about seeing so little of his method of operating. But in this case eyesight was really of no use. You must be able to diagnose the various organs by the sense of touch. This of course might be cultivated by practice on dead bodies, but every one must learn for himself through the actual operation. The risks of the operation are two in number—*Firstly*, Tearing organs which you did not intend to tear. The drainage-tube introduced by Koeberle and Keith minimizes this danger. In one case he tore a hole right through the rectum. He put a long drainage-tube right through the wound, and although the faeces passed through the tube for weeks, the result was an uninterrupted cure. *Secondly*, Hæmorrhage. This is sometimes tremendous and very alarming. In the first place, pack the pelvis as full as it can hold with sponges, and wait for fifteen minutes. If it continues, wash out the cavity with a solution of the perchloride of iron. This is somewhat risky, and the best results will follow from inserting a drainage-tube, and teaching your nurse to pump out the blood from the pelvis as it collects.

Myoma or Fibroids of the Uterus.—These are usually found in women about the ages of 36 to 40. Sometimes they are harmless, but in other cases we find them causing a great amount of trouble. Where they set up a weakening and persistent hæmorrhage, operation is made necessary. In some patients they may have assumed enormous proportions, as in one case on which he operated, where the tumour weighed 60 lbs., and required an incision of 21 inches to get it out. He advocated early removal, as if you operate early you get over this difficulty, and, besides, the very great danger attending pregnancy under such a condition.

On the motion of Dr Halliday Croom, seconded by Dr Berry Hart, a most enthusiastic vote of thanks was accorded to Mr Lawson Tait, who, in reply, said that he felt deeply two regrets, which were alternative—either that he could not be back amongst

them as a student, or that he could not be one of their teachers. He could not stand there even for a few moments, as it were in the shoes of his dead master, without feelings of emotion that almost overwhelmed him, and if he could not adequately thank them for their patient hearing, and Prof. Simpson for the high compliment he had paid him, it was not for want of words, but for their too great fulness.

III.—ON DERMATITIS FEROX.

By J. L. MILTON, Senior Surgeon to St John's Hospital for Diseases of the Skin.

(Read before the Willan Society, 3rd December 1885.)

THE disease to which I have ventured to give the name of Dermatitis Ferox is, in its more developed form, so exceeding rare, that as yet no one to whom the drawing of a case taken at the hospital has been shown has recognised the affection; I therefore thought that a brief description of the complaint, accompanied by the history of a case, might not be unacceptable to the profession.

The outbreak seems in every instance to take the form of one or more scattered patches on the face, hands, or upper part of the chest, and sometimes on all three together. The patches, at first only reddish, speedily become of a vivid red, and then red mixed with brown, the cuticle turning dry and crumpled, and then peeling off, to be replaced by another layer, which in its turn shares the same fate. Now and then a small part of a patch may ulcerate, but this is an extremely rare complication, and usually the morbid state disappears by the gradual declension of the redness and slow reproduction of unhealthy cuticle, which is also little by little replaced by a more normal covering. There is usually no discharge, nothing in the shape of a scurf or crust. The patches, as a rule, form very slowly, and it is only after a time, and when they have become rather extensively developed, that the health begins to suffer; but in other cases a difference in the process may be noticed in so far that the eruption takes place rather quickly, and that the health fails within a few days after its appearance. Sometimes, when the disease attacks the side of the face in the male, firm crusts will form, so that at the end of a week the case looks like one of rather advanced erythematous sycosis. Such crusts may crack, and under the lens small spots can be seen, which look as if thick serum were exuding from them, but in the early stages at least there is no weeping, as in eczema. The site of each crust is surrounded by a red inflamed ring, much broader than is seen round a sycosis patch, which is also infinitely slower in forming. In every case which has yet come under my care the affection of the skin has shown itself first, and the constitutional disturbance later, sometimes much later.

The patches are in general quite isolated, and at first not very large, the predominating shape, if anything with such an irregular outline can be said to have a shape at all, being a ragged oval. Usually they are, as stated, of a lake colour, and this seems to betoken a milder phase; in two cases, where the hue might very fitly have been compared to carmine, the constitutional disturbance was out of all proportion to the extent of skin affected. After a time, and particularly when they are seated on the face, two or more patches gradually fuse, and take on a dark, angry hue, which gives the part the look of having been burnt with fire. Now and then they assume a papular form, and then resemble flattened, aggregated lichen spots; these appear to be overlaid by a cuticle not unlike the horny covering of lichen. On other patches the cuticle, after taking on a brown crumpled look, is cast off, as in the rising stage of eczema. In a third set of patches the scarf skin may be fairly adherent.

The constitutional symptoms point to much and often grave disturbance of the health. General uneasiness, feeling of being unwell, sometimes of the system having taken the alarm, loss of appetite, nausea, headache, inability to walk straight, and prostration, are perhaps the most prominent and frequent among the many signs which arrest the attention of the practitioner.

It will thus be seen that, while the affection in some features resembles acrodynia and erysipelas, it is yet distinct from both. It runs a much slower course than either, and in it the constitutional symptoms come on much later. The phlyctenæ, pustules, cedema, and perspirations of the former are wanting. When it attacks the face it is accompanied by none of the swelling and closing of the eyelids which so distinctly mark erysipelas. It is sometimes attended, too, by the formation of huge bullæ at a distance from the chief site of active disease. Though, perhaps, in this complaint the skin is just as much poisoned by the air as it is by the aniline and arsenic dyes in a case of glove or sock poisoning, yet we do not find the sudden swelling, heat, tenderness, and subsequent discharge of serum seen in the latter affection. On the contrary, every one of these symptoms may be absent; indeed, redness and exfoliation are usually the sole local signs. But while I never saw much internal disturbance, and particularly internal disturbance culminating in delirium, from the aniline dye affection, I have met with both under a very pronounced form in this kind of dermatitis. And no form of poisoning of the skin which I have either read of or seen, or any variety of erysipelas known to myself, is ever followed, as dermatitis ferox sometimes is, by a raised, fused, red, papular eruption. For all such reasons the latter seems to me a distinct individual complaint.

The case spoken of is as follows:—

Martha S., aged 24, a tall, strongly-built woman, was admitted into St John's Hospital, 17th April 1885, in a state of great prostra-

tion and suffering. The face, from the roots of the hair to below the chin, was of a fiery red, extremely stiff, and very slightly swollen. The lips, which were covered with sordes as bad as in any case of typhus, were superficially eroded all over, and, according to the patient's account, bled so much when she attempted to eat, that she frequently turned sick from this cause. The combined effect of these symptoms on the face was to give it a most singular and revolting look. There was a slight discharge from the right ear.

According to her account, which was only with great difficulty extracted from her, the woman being in a state approaching coma—she was a hawker of oil and floorcloths—and therefore of necessity much exposed to the weather, which, I may remark, had long been very inclement, bitter east winds prevailing for some time before. She was in the habit of carrying the cloth in her arms, and the first appearance of the eruption was on the flexor side of the tip of one finger. peculiarly exposed to the action of the material in question. The disease had been ascribed to this fact, the influence of which, however, is very doubtful, as it was impossible that the other fingers could have altogether escaped, and they were not affected at that time. I may remark, however, that with regard to both dates and symptoms, her narrative, not only at this period, but even long after her health had materially improved, frequently contradicted itself; not that I believe she ever had any wish to deceive us; on the contrary, she always appeared truthful and communicative enough. The difficulty was that her memory evidently failed her on some points, and that she had not observed very accurately on others. As well as we could make out, the spot on the finger just alluded to and a patch on the right cheek formed several months before her admission. The latter gradually spread till the whole face was covered, without a break in the redness. About three weeks prior to her entrance the morbid process had gained the lips, and had affected them as I have described. Rather more than three weeks before this again the flexors of the fingers began to crack, and deep keens formed in them. At the time of her admission there were several of these cracks, and the edges of some of them seemed nearly in a sloughing state, almost as if each had been the site of a bad whitlow. On the right foot and underneath the ball of the great toe, extending for some way along the sole of the foot, was a large bulla about half filled with what seemed to be pultaceous matter, though the contents could not be accurately determined on account of the skin not having broken. On its being opened the next day a most offensive semi-purulent discharge came away. There was a similar but smaller bleb on the left foot, both being of very irregular shape. The patient was very hoarse, which might, however, be due to her calling. Syphilis had been suggested as the cause of all this, but there was no history of syphilis, and there were no symptoms of it.

She was put on 20 minim doses of tincture of steel every four hours, with a gentle aperient at bedtime; the day following the dose was raised to 30 minims. Light diet was ordered, with a free allowance of wine; the face to be covered with benzoated zinc ointment, and the lips to be repeatedly washed with solution of chlorate of potass, 5 grains to an ounce. For four days she seemed to go on extremely well; but on the fifth day, 21st April, an extensive crop of papules, exactly like those of lichen ruber, broke out on the backs of the hands and arms, accompanied by some failing of the appetite. The following morning I noticed that she was very depressed, and learned that she had been crying without any reason. During the ensuing night she was very restless, and passed a great deal of blood by the bowel; she had also copious bleeding from the nose, which was stopped by plugging. The night after this she turned delirious, attacked and overpowered two nurses, and was only prevented by the efforts of the bath keeper from rushing out into the street. Next day when I saw her she was much worse than at any previous visit; the lips were thickly crusted with sordes, and she rambled continually. She had torn off all the dressings laid on the lichenous spots, and had so violently scratched these that it would have been almost impossible for any one, who had not seen the case before, to make out the character of the eruption. The stools were passed involuntarily, along with an immense quantity of flatus and urine; her pulse was quick and weak, and her strength seemed failing. She was ordered 5 minims of tincture of aconite every four hours, along with 20 of spirit of chloroform and 15 of syrup of red gum; a small bottle of Guinness's best stout night and morning, as much light soup and mutton broth as she could be induced to take, and brandy if there seemed to be need for it.

This change in the treatment was at first followed by beneficial results. On the morning of the 25th she was quieter, and there were fewer sordes about the mouth; the redness of the face was also declining. She had taken the stout freely, and expressed a wish for some solid food; the stools no longer passed involuntarily. With all this the eruption had spread on the neck, arms, and backs of the hands, over which were scattered an extraordinary number of lake-coloured erythema patches, looking as if she had an eruption of chilblains. A large bulla, nearly if not quite 2 square inches in area, had formed on the back part of the sole of the right foot. During the whole time her tongue had kept clean, and this continued to be the case. The day after the stools were still loose, but there was no return to involuntary passing of them. The colour of the face had abated, but the rash was extending, and crops of flattened lichen papules were continuing to break out on the patches, while more large blebs were forming on the soles of the feet. She was still very violent, and could with difficulty be kept in bed. The dose of aconite tincture was increased to $7\frac{1}{2}$ minims; the violence, however, continued all through the next day, and up

to four o'clock the following morning, when she fell into a deep sleep, from which she awoke, comparatively speaking, quite rational. She was ordered to continue the stout and jelly, but the aconite was suspended, unless a fresh necessity for it should arise; the steel, which had been interrupted during the height of the attack, was renewed.

She now went on very well for five days, when she had an attack of delirium, which lasted six hours; this was the last of the acme. On the 9th of May I noticed that the skin of the face and hands was peeling off, and on further search found that nearly every part of the arms and body was undergoing the same process; the erythema patches and the skin at first spared by them being alike involved in one general dermatitis. Her appetite, however, was now excellent, and her strength returning so fast that she asked to be discharged; and she accordingly left four days after, being directed to continue her steel and attend as out-patient.

On the 18th of May, however, she reappeared with every sign of a bad relapse. The colour of the eruption had deepened again, and the mingling of this fresh hue with the brown of the dying epidermis gave the face and neck the look of having been severely scorched. She was low, hysterical, and scarcely able to stand; her feet were very painful, and on the wrists were several patches of almost a carmine colour. She reported herself as having been most attentive to her case; asserting that she had remained quietly at home, had taken her medicine quite regularly, and had carried out all the instructions given her; nor did there seem any reason to doubt her statement. The weather, too, had been so very bad that she would scarcely have been tempted out of doors. There being no vacant bed at the time, she could not be re-admitted then, and later on declined to come in, having to look after her household; she was, therefore, simply directed to continue her treatment. I did not see her again till the 1st of June, when her condition had very little altered, the cuticle still looking dry, brown, and withered, and the patient herself low, weak, and hysterical. Some of the patches on the body and arms were better, and a certain, very slight, degree of eczema, which had appeared on some of them, was now gone. With all this, she soon after once more began to improve in health, and by the middle of the month reported herself as feeling quite well, one result of which was that I had to threaten, more than once, to discharge her for irregular attendance. She had kept up the use of the steel, and was directed to continue it, and to have a vapour bath once a week, a recommendation which she carried out very indifferently. At a visit paid on the 2nd of July she said she felt quite well, though she really seemed to be very ill, while the skin looked nearly as bad as ever, and on the 27th was a great deal worse again, that on the face especially having a most angry appearance, and the fronts of the arms being almost covered with lake-coloured spots. Some part of this change was perhaps due to the

violent heat which had prevailed during the two foregoing days. A full dose of calomel at bedtime, to be repeated three days after, and followed up each time with a black draught, seemed to do her good, and at her visit ten days later the patches looked paler, and there was much less dying cuticle. From this time I saw no more of her. She attended very irregularly during my absence on a long holiday, neglected her vapour bath, and finally died, December 2nd, of pneumonia.

This was the second instance in which the health seemed to be irretrievably shattered by the affection—a former patient, previously a strong man, having to resign his profession and go abroad. A nurse who had known this patient, and who visited him during his illness, said “no words could convey an idea of the ravages it had made in a fortnight.” With the exception of the person whose case forms the subject of this paper, all the patients were middle-aged people in easy circumstances.

IV.—SEX IN CONNEXION WITH PULMONARY PHTHISIS.

By ALEXANDER JAMES, M.D., Assistant Physician, Royal Infirmary, Edinburgh.

WHEN we look for data upon which to form an opinion as to the relative frequency of pulmonary consumption in the sexes, we find results which are decidedly conflicting. From some writers, as Williams,¹ Bennett,² Pollock,³ it might be inferred that the disease preponderated in males; others, as Louis,⁴ Laennec,⁵ Walshe,⁶ Hutchinson,⁷ give evidences of an increased liability to it in the female sex; whilst a third, as Niemeyer,⁸ Bristow,⁹ Ruehle,¹⁰ Andrew,¹¹ consider that no distinction as regards relative frequency can be drawn.

Further, not only have such contradictory statements been made, but by many physicians of eminence theories have been put forward in explanation of a supposed greater liability to consumption in one or the other sex. Those who believed it more common in men ascribed it to occupations and greater liability to exposure, and, indeed, to all injurious influences; whilst among those who

¹ *Pulmonary Consumption*, p. 294.

² *Reynold's System of Medicine*, p. 546.

³ *Elements of Prognosis in Consumption*, pp. 245 and 256.

⁴ *Researches on Phthisis* (Sydenham Society translation), p. 480.

⁵ *Diseases of the Chest* (Sir John Jules' translation), p. 307.

⁶ *Diseases of the Lungs* (4th edition), p. 462.

⁷ *The Spirometer, Stethoscope, and Scales Balance, etc.* (1852), p. 46.

⁸ *Text-Book of Practical Medicine*, vol. i. p. 216.

⁹ *Theory and Practice of Medicine* (2nd edition), p. 423.

¹⁰ *Ziemssen's Cyclopaedia*, vol. v. p. 506.

¹¹ *Brit. Med. Journal*, i. 1884, p. 709.

believed it more common in women a very great difference of opinion has existed. By some it has been ascribed to something inherent in the individual, by others to the external conditions or surroundings. By the former such expressions as "greater original delicacy of constitution," "less power of resistance," "greater tendency to anæmia," are made use of as regards women, and the evil effects of confinements, lactation, etc., are brought into prominence; whilst by the latter, the more sedentary life of women, their bad physical education, the wearing of stays, exposure of the upper part of the chest, etc., have been looked upon as the more probable cause.

In order to discover if any data of use in elucidating this question could be obtained by studying the relative frequency of the affection in the sexes in the different districts of Scotland and England, I have examined the reports of the Registrars-General for a series of years on this subject, and the results thus obtained I now propose to consider in detail.

In the first place, if we compare the mortality from phthisis in Scotland with the general mortality in the sexes, we find results which are sufficiently conclusive. The Registrar-General's returns for Scotland for the ten years 1871 to 1880 average as follows:—

	Males.	Females.
Total yearly deaths per 1000, ...	22.58	20.73
Yearly deaths from phthisis per 1000, ...	2.36	2.52

Inasmuch, then, as the total death-rate in Scotland is greater in men than in women, whilst the death-rate from phthisis is greater in women than in men, some special proneness to this disease, as compared with other morbid conditions, ought to be found existent in women.

But, secondly, the proportion of female to male phthisis mortality is not the same, but, indeed, is markedly different in different localities. This is shown in the following table, which represents the average proportionate mortality from phthisis in the sexes, in the five districts of Scotland for the ten years 1871 to 1880.

Deaths from phthisis per 100,000 persons living—

Principal Towns.		Large Towns.		Small Towns.		Mainland Rural.		Insular Rural.	
Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.
298.9	311.7	277.8	315.6	227.8	249.4	172.3	177.6	201.0	163.4

From this we see that male phthisis is most marked in the towns, less in the insular rural, and least in the mainland rural; while female phthisis, although also most marked in the towns, is less in the insular rural than in the mainland rural. The relative liability to phthisis can, however, be better demonstrated by

reducing these numbers to percentage, and if we do this, we find that for every hundred female deaths from phthisis there are in the

Principal Towns.	Large Towns.	Small Towns.	Mainland Rural.	Insular Rural.
94·2	88·0	91·3	97·0	123·0
male deaths.	male deaths.	male deaths.	male deaths.	male deaths.

This shows that while in the town districts women are more liable to phthisis than men, in the mainland rural they are much less liable (*i.e.*, the liability of the sexes is nearly equal), while in the insular rural districts the reverse is markedly the case.

If, now, we take the proportionate male to female mortality from all causes for the same periods, we get the following result:—

Percentage of male and female deaths in the five districts for the ten years 1871 to 1880. Average—

Principal Towns.		Large Towns.		Small Towns.		Mainland Rural.		Insular Rural.	
Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.
2·775	2·467	2·575	2·341	2·167	2·003	1·757	1·680	1·665	1·557

If, now, again we take these numbers in relative percentages, we find that for every hundred female deaths from all causes there are in the

Principal Towns.	Large Towns.	Small Towns.	Mainland Rural.	Insular Rural.
112·4	109·9	108·1	104·5	106·9
male deaths.	male deaths.	male deaths.	male deaths.	male deaths.

These results show that the general mortality is greatest in the towns, much less in the mainland rural, and least in the insular rural; and that the female mortality is in all the districts less than the male, the difference being greatest in the towns, less in the insular rural, and least in the mainland rural.

As being of further use in connexion with our subject, let us now take the birth-rate in the same districts.

Percentage of births to the population in the five districts for the ten years 1871 to 1880. Average—

Principal Towns.	Large Towns.	Small Towns.	Mainland Rural.	Insular Rural.
3·780	3·905	3·551	3·092	2·591

As was to be expected it corresponds to the death-rate, *i.e.*, greatest in the towns, less in the mainland rural, and least in the insular rural.

From these tables it will be seen—1. That in the towns where the general mortality, the phthisis mortality, and the birth-rate are highest, there exists as regards general mortality the greatest excess of male deaths, and as regards phthisis mortality the greatest excess of female deaths.

2. In the mainland rural districts where the general mortality

and birth-rate are very low, and where the phthisis mortality is lowest, there exists as regards general mortality a less excess of male deaths, and as regards phthisis mortality a less excess of female deaths.

These differences between the town and mainland rural districts might readily be explained as follows:—In the latter the surroundings of the people will be better, so that the general health will be superior. Further, the occupations of the sexes will be more equal—the out-door life predominating, and the risk of injury to women from what may be termed errors of clothing will be practically non-existent. Lastly, the birth-rate being low will diminish the depressing effects of repeated pregnancies and lactation.

Still it is to be observed that the mainland rural districts, although so favourably placed, show a preponderance of female phthisis. Are we to suppose that this indicates that there is in women as compared with men that which has been vaguely termed a “less power of resistance” to disease? If so, how can we associate this with the fact that the general mortality is less in the case of women? Is it not rather the result of the fact, that although both sexes are favourably placed as regards phthisis in the mainland rural districts, yet women’s life being more in-door is the less so of the two?

But now let us consider the case of the

3. Insular Rural Districts. In these the general mortality and birth-rate are lowest, as is also the female phthisis mortality; but the male phthisis mortality, higher than that of the mainland rural, though less than that of the towns, greatly exceeds the female phthisis mortality, being in this respect quite exceptional.

How, then, can we say that the greater liability of women to phthisis as a whole is due to this so-called “power of resistance” being less than in man, *i.e.*, is due to something innate in her constitution? Is it not rather simply due to the circumstances in which society places her, circumstances most unfavourable to her in town and less so in country districts? Or is it not the case that the comparative freedom from phthisis which women enjoy in the insular rural districts is more apparent than real, *i.e.*, that men are here placed under exceptionally unfavourable conditions, and consequently suffer correspondingly, so that the greater innate tendency of women to the disease is more than counterbalanced?

Before considering this matter further, let us direct attention to the reports of the Registrar-General for England. In those no groupings into principal, large, and small towns, and rural districts as in Scotland are made, but I have endeavoured to obtain what may fitly be compared with the town and rural districts of Scotland by taking London to represent the former, and the counties of Dorset, Sussex, Hereford, Rutland, Huntingdon, Suffolk, and

Wiltshire, to represent the latter. These selected counties are agricultural, and show the lowest death-rates of all the English counties,¹ they may therefore be reasonably compared to the mainland rural districts of Scotland.

In the first place, however, let us note the proportion of male to female deaths (*a.*) from all causes, and (*b.*) from phthisis, in England as a whole.

For the ten years 1872 to 1881 the average was as follows:—

	Males.	Females.
Total yearly deaths per 1000, ...	22.0	19.4
Yearly deaths from phthisis per 1000, ...	2.14	1.99*

It will here be observed that the general and phthisis mortality are both lower in England than in Scotland, but what we have specially to concern ourselves with is the fact that in England, whilst the general mortality as in Scotland shows an excess of males, the phthisis mortality does so also, differing from what we have seen to be the case in Scotland.

Reverting now to the selected town and county districts, we find that the average deaths from phthisis per 100,000 living for ten years (1871 to 1880) were in

London.		Selected Counties.	
Males.	Females.	Males.	Females.
304.2	208.4	186.2	192.1

If, now, we reduce these numbers to percentages, we find that for every 100 female deaths from phthisis there are in

London.	Selected Counties.
145 male deaths.	96.9 male deaths.

These results are somewhat different from what we might have expected, for they show in London, a typically urban district, a large excess of male phthisis. The selected counties, however, like the mainland rural districts of Scotland, show a slight excess of female phthisis.

Let us next consider the proportions of male to female deaths from all causes in these districts.

Percentage of male to female deaths from all causes for ten years 1871 to 1880 (average)—

London.		Selected Counties.	
Male.	Female.	Male.	Female.
2.48	2.07	1.89	1.70

Again, taking these numbers in relative percentages, we find that for every 100 female deaths from all causes there are in

London.	Selected Counties.
119 male deaths.	111 male deaths.

These results correspond with those of the towns and mainland rural districts of Scotland in showing a greater general mortality

¹ See Supplement to the 45th Annual Report of the Registrar-General, p. 18.

² A table given by Walshe, *ibid.*, p. 463, seems to show that this excess of male phthisis in England and Wales has not always existed.

of males, most marked in the urban district. The excess of male deaths, however, is much greater in the English returns.

To carry out the method pursued previously, I now add the birth-rate in these districts.

Birth-rate for ten years (1872 to 1881) per 1000 (average)—

London.
35·4

Selected Counties.
30·4

These, of course, correspond with what we have noted in Scotland, giving a higher birth-rate in town districts.

When, now, we compare the Scotch and English results, we find on the one hand a fairly close correspondence. In the towns the birth-rate, general mortality, phthisis mortality, and proportionate male to female general mortality are high. In the mainland rural districts of Scotland, and in the selected counties of England, these are low; and further, these districts correspond with one another in showing a similarly slight excess of female phthisis. In one important respect, however, the results in the two kingdoms do not agree,—as London, a town district, shows a marked excess of male phthisis.

What, therefore, can we now say in answer to the question as to the relative liability of the sexes to phthisis?

It is evident that as regards data on which to form an opinion on this subject, those localities in which the general mortality and the phthisis mortality are lowest will give the most trustworthy results. Where these two averages are high, there must be some cause, some condition interfering with health, possibly affecting both sexes equally, much more probably affecting one specially. As to which sex is acted upon most injuriously in towns like London or Manchester, or in counties like Cornwall or Staffordshire, there need be no doubt; and although I cannot explain why a town like Manchester should show an excess of male, and Glasgow of female phthisis, I believe that the excess of male phthisis in the insular rural districts of Scotland can similarly be explained as the result of surrounding circumstances. The conclusion, therefore, which I would draw is that women are rather more liable to the disease than men.

But how is this? Is it, as stated before, the result of something innate in the constitution of woman, or is it the result of her surroundings in the social organism?

To the latter view the data which we have discussed at page 824 would rather incline us, inasmuch as phthisis is a disease which emphatically tends to affect those who lead an indoor life. To draw a conclusion on this point, however, I do not think we are entitled, since similar discrepancies are met with in connexion with physiological and pathological conditions with which the indoor life can hardly be supposed to have so much to do. For example, the proportion of male to female births is different in the town and rural districts of Scotland and England.

Proportion of male to female births for ten years 1871 to 1880.
Females = 100

Scotland—

Towns.	Mainland Rural.	Insular Rural.
105·8 male births.	105·9 male births.	107·6 male births.

England—

London.	Selected Counties.
103·8	104·0

Again, similar differences in statistical results seem to occur in connexion with insanity, at any rate in Scotland. Thus, in districts which may be regarded as corresponding with the registration ones, the proportion of male to female cases varies considerably, as shown by the following table, which gives the numbers of intimations during the ten years 1873 to 1882 (average):—

	Edinburgh (chiefly Urban).		Peebles, Roxburgh, and Selkirk Counties (chiefly Mainland Rural).		Argyll (chiefly Mainland and Insular Rural).	
	Male.	Female.	Male.	Female.	Male.	Female.
Average number of pauper lunatics per 1000 population.	64·5	68·2	34·0	39·8	68·6	66·6
Proportion per cent.	100	105·7	100	116·3	100	97·6

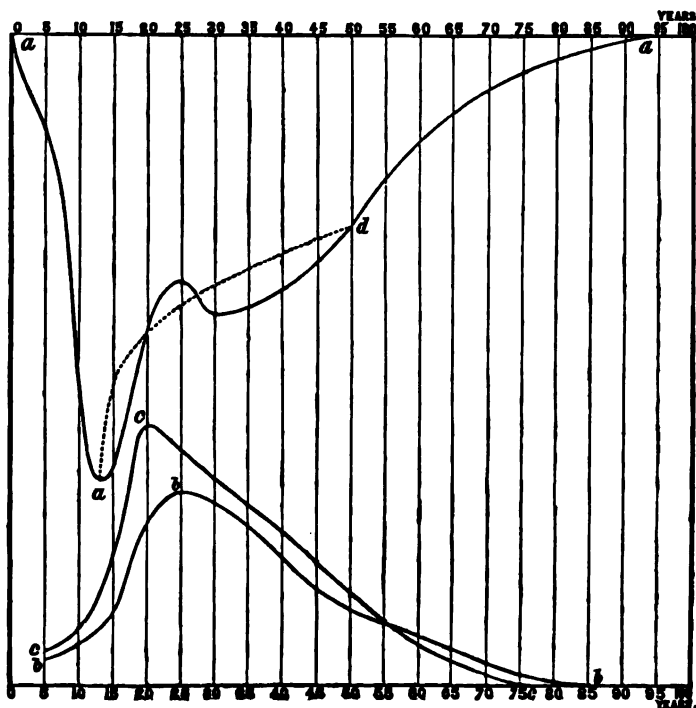
This shows that in the district (Argyll), where insanity is most frequent, there is a preponderance of male cases; in the districts (Peebles, Roxburgh, and Selkirk) where it is least, a great preponderance of female cases; and that in a district (Edinburgh) which occupies, as regards frequency, an intermediate position, the proportion of male to female cases is intermediate. On the assumption that women are innately weaker than men, one could readily explain the difference between these proportions in the county of Edinburgh on the one hand, and those of Peebles, Roxburgh, and Selkirk on the other, as being due to business cares, alcohol, etc., causing greater disturbance in males in the former district. But this would not account for the state of affairs in the Argyll district.

The evidence derived from insanity, therefore, might seem to indicate that the slightly greater tendency of women to phthisis is not the result of her more indoor life, but of some inherent weakness in her constitution, and the fact that cancer is also more frequent in women would seem to corroborate this. Still, as before mentioned, women are longer lived than men; and although this might be explained as being due to their surroundings, whilst rendering them rather more liable to phthisis, yet shielding them to a greater extent from other morbid conditions, we might also suppose that these surroundings, entailing less sunlight, oxygen, exercise, etc., are similarly capable of rendering them more liable to insanity and cancer.

The fact of a country life, with its low general mortality,

phthisis mortality, and birth-rate, showing a greater proportion of male to female births than a town one, could readily afford material for speculation as regards the question of the relative proportion of synthetic to analytic processes in the sexes, and the bearing of this on phthisis. Such speculations, however, are not within the scope of this paper.

When we next consider the question of the relative age at which phthisis is most apt to occur in women, we find, as might be expected, some difference from the other sex. As the tables given by Williams¹ and Hutchison² show, and as is borne out by the Registrar-General's returns,³ the disease is apt to occur and reach a maximum earlier in women. Williams's table shows that whilst between 10 and 30 more women are attacked than men, after 30 the reverse is the case; and although, as the curve shows, the Registrar-General's returns do not quite correspond to this, they give sufficiently clear evidence of an important difference in this respect.



a a General mortality, after Quetelet. It has been constructed for both sexes, but the dotted line a d shows the modification for women.

b b b Male mortality from Phthisis.

c c c Female mortality from Phthisis.

¹ *Pulmonary Consumption*, p. 296.

² *The Spirometer, the Stethoscope, and Scale Balance*, etc., by John Hutchinson, D. London: 1852.

See Curve, Plate I.

Why phthisis occurs earlier in woman may be probably explained by the fact of the earlier cessation of growth in her case as compared with man. Every one knows that, as Spencer puts it, "a girl develops in body and mind rapidly, and ceases to grow comparatively early. A boy's bodily and mental development is slower, and his growth greater. At the age when the one is mature, finished, and having all faculties in full play, the other, whose vital energies have been more directed to increase of size, is relatively incomplete in structure, and shows it in a comparative awkwardness, bodily and mental." That is to say, in the male the excess of assimilation is later in being exhausted, hence the later development of the phthisical tendency. But further, it is noteworthy that, as the curve shows, after the age of 50 or 55 the tendency to phthisis on the part of woman becomes less than that of man. For this there are several reasons. In the first place, it is to be remembered that with the cessation of menstruation, that is to say, with the cessation of the capability for reproduction, a less expenditure will occur. Hence a greater tendency on the part of women¹ to produce fat as age advances; and this, together with the tendency to emphysema, with which it is usually associated, indicates a condition which renders the lungs less liable to the disease. Further, the fact that after 45 or thereabouts woman is shielded from the exhausting effects of pregnancies, lactation, and even of menstruation itself, is to be considered.

The greater liability to phthisis from which women suffer seems to have associated with it a correspondingly diminished duration; i.e., a correspondingly increased acuteness and severity as compared with men. Whether this be the result of a greater inherent weakness of constitution in woman, or of her circumstances and surroundings in the social organism, we need not again discuss. It is remarkable, however, that Walshe² concludes that, although in hospital complete removal of symptoms is more common in males, the general result of treatment is more satisfactory in females. Seeing that female cases are less likely to come under treatment at an early stage, and that in hospital they are placed in precisely the same circumstances as males, this observation might appear rather opposed to the view of there being a greater inherent weakness in women. With the observation, however, my own experience leads me to coincide; and I believe it to be explained by the fact of women being more accustomed than men to indoor existence and want of exercise, and consequently accommodating themselves more readily to a more or less confined hospital life.

That pregnancy is looked upon by the laity as exercising a beneficial effect on phthisis is well known; and the works of Graves, Copland, Walshe, Pollock, etc., have given to this notion

¹ Compare Paget, *Surgical Pathology*, p. 82.

² Walshe, *ibid.*, p. 468.

a scientific value. Of course there are not wanting observers who not only deny this mitigating influence, but actually assert that the reverse is much nearer the truth, i.e., that pregnancy renders the phthysical symptoms more severe. To such a conclusion Grisolle,¹ Lebert,² and Spiegelberg³ have to a greater or less extent arrived, the latter stating also that the risk of hæmorrhage in phthisis is increased by pregnancy. In the face of such contradictions, it may appear questionable if it is worth while discussing this matter further, inasmuch as if it is doubtful whether pregnancy affects phthisis for better or for worse, any generalizations as to the etiology of phthisis obtained from a study of its connexion with pregnancy can hardly be regarded as resting upon a secure basis. If, on the other hand, the common view of the phthysical symptoms and conditions being mitigated by pregnancy contains in it any truth, information as to the wherefore is evidently correspondingly valuable. On consideration of the subject, I am disposed, to a considerable extent, to believe in the old view; and my reasons, apart from individual experience, are—

First, that in estimating the effects on phthisis of pregnancy, it is difficult to eliminate those of childbirth and lactation, which are undoubtedly injurious; and, secondly, that the period of life at which pregnancy occurs most frequently is that at which pregnancy is most apt to occur, and that consequently the evil effects of pregnancy in a phthysical individual may be more apparent than real. Thirdly, that pregnancy can act beneficially or injuriously in connexion with the onset and course of other diseases. Thus Clouston⁴ states that although there is an insanity of pregnancy, yet there is no period in the life of a woman in which she is less subject to insanity. Further, I have been fortunate enough to witness the effects of consecutive pregnancies in alleviating the attacks in spasmodic asthma, a disease on which the influence of pregnancy for good when manifested is obviously much more patent than in the case of phthisis, which, steadily progressive, as it is too apt to be, renders appearance of amelioration or aggravation less marked. Conversely the casual connexion of a prejudicial character between pregnancy and skin and renal diseases, which is allowed to exist, may, as we shall see later on, be made to afford further evidence in favour of the common view.

In explanation of this real or supposed influence, various theories have been brought forward, all of which, however, may be classed in two divisions—(a) local, (b) general. Under the first head the beneficial effects are to be attributed to the abdominal tumour mechanically affecting the respiratory apparatus; under the second, to the general effects on the system induced by pregnancy.

¹ *Archives générales de Médecine*, serie iv. tome 22, 1850, s. 41.

² *Archiv für Gynäkologie*, Band iv. s. 468.

³ *Handbuch der Geburtshilfe*, p. 285.

⁴ *Mental Diseases*, p. 517.

According to the local theory, the abdominal tumour, by pushing up the diaphragm and interfering with its action, induces in the lungs a condition allied to that in which they exist in emphysema, heart disease, ague cake, etc., that is, congested, watery, and functionally more active.

Unfortunately for this view, however, the respiratory capacity, as ascertained by the spirometer, is not so markedly diminished in pregnancy as one would expect. Indeed, Wintrich, Fabius, and Küchenmeister,¹ concluded that it is practically the same as in the non-pregnant state; and Dohrn,² although he found out of 100 cases that in the majority there was a certain amount of diminution during pregnancy, yet that in some there was, twelve or fourteen days after delivery, a respiratory capacity equal to or rather less than there had been at the ninth month. Of course it may be readily supposed that a woman has not the same muscular strength a fortnight after childbirth as she had during pregnancy; and that, consequently, spirometry at these times may not give results sufficiently trustworthy for comparison. There can be no doubt, however, that the diminution in the respiratory capacity during pregnancy is nothing like so great as might have been expected; and that, therefore, as an explanation of the ameliorating effect of pregnancy on phthisis, this local theory has not the importance which it might otherwise have had. It is nevertheless very valuable, and the more so from the fact that during pregnancy the daily excretion of CO₂ is increased, as this will intensify the effects of any diminution in the breathing surface.

According to the second theory, this mitigating influence is the result of certain changes in the system which pregnancy induces. These changes are numerous and important. We know, for example, that the blood is markedly altered in composition, being more watery, and having its corpuscular elements so altered as regards number as to present a relative increase in the proportion of white corpuscles. The heart is enlarged, the left ventricle seems to be specially hypertrophied, and the arterial tension seems to be increased.³ Trophic changes occur in parts remote from the generative organs; e.g., the mammary glands enlarge, and this even in the absence of direct nervous connexion. Hence we may suppose that the trophic processes in the lungs may be in some way influenced.

Knowing also that pregnancy has been looked upon as tending to affect the kidneys as injuriously as it does the lungs beneficially, and presuming that the view which supposes that this is due to some cause other than the pressure of the uterus on the renal veins, is the correct one, we might further suppose that owing

¹ For these see *Monatsschrift für Geburtskunde*, Bd. xxviii. (1866), p. 457.

² *Ibid.*

³ See Angus Macdonald, *Heart Disease during Pregnancy*, pp. 6 to 23.

to the functional connexion between lungs and kidney, any condition which affects in the way of rendering more difficult the functioning of the one will exercise a stimulating effect on the other.

Lastly, we have to remember that just as Pollock says of tubercle, "that every agent capable of converting it from its course adds prolongation to its time,"¹ the beneficial effects of pregnancy in phthisis may be likened to those produced by counter-irritation. Pregnancy may therefore mitigate the pulmonary symptoms in the same fashion as does a blister, seton, or even the occurrence of a complication such as ulcerated intestine, or fistula.

Parturition is an act which is always accompanied by danger, and although the phthysical condition seems frequently to render its performance more rapid and less exhausting, yet the risk of hæmorrhage, the occurrence of a certain amount of fever, and the fact that it, along with uterine involution, necessitates sudden and important changes in the amount and composition of the blood, and in the functions of the excreting organs, seems to render its performance in this disease more than usually hazardous. Moreover, there is reason to believe that the condition of the generative passages in the post-partum state may lead, in tubercular cases, to the development there of tubercular granulations, and so add to the exhaustion of the patient.²

It is important to observe that, as stated by Pollock,³ it is rare for a pregnant phthysical patient to die undelivered. Even in very acute cases, life is usually prolonged until labour has occurred. Lactation is invariably to be prohibited in the phthysical, as dangerous alike to mother and child.

The question of marriage for consumptive individuals is one which frequently presents itself, and although (perhaps fortunately) medical is as little likely to be followed as any other advice in this matter, its importance generally is immense. It has two aspects, a social and an individual one.

From the first of these points of view, all observation points to injury done thereby. The constitutional debility will certainly be transmitted; and it is well known that cases hereditarily phthysical present an earlier development and a more rapid course than others. In this aspect, therefore, the question of marriage is to be strongly negatived.

From the individual point of view, however, the question may be answered differently. Phthisis is an affection in which remissions of symptoms are apt to occur; and inasmuch as these afford valuable opportunities for treatment, their occurrence should be favoured by every means in our power. We have also to remember that the progress of this, as of other diseases, is often powerfully affected by the mental condition, "*spes phthisica*"

¹ Pollock, *ibid.*, p. 388.

² Walshe, *ibid.*, p. 445.

³ Pollock, *ibid.*, p. 299.

notwithstanding. Any depressing emotion, therefore, such as would be entailed by actually negating the question of marriage, is almost certain to minimize the good obtained from these remissions. Again, in the case of women the possibly beneficial influence of pregnancy is to be considered; and although this may be counterbalanced by the perils incident to delivery and lactation, yet these dangers may be greatly diminished, and in comfortable circumstances, in cases where the amount of lung substance destroyed is so limited that the cicatricial contractions can be compensated for by emphysema of the surrounding lung tissue, the results may be on the whole favourable.

Whilst admitting this, however, I believe that, as before stated, under no pretext should lactation be permitted. No doubt, as Rueble¹ says, a woman in such circumstances may be induced to take and assimilate a very large quantity of food, and benefit herself for the time as the result of the laudable desire to benefit her child. The risk, however, of ultimate evil results to both more than counterbalances the benefit to be obtained thereby.

Menstruation in the prephthisical stage may be fairly normal; but, as in any other debilitated condition, it is often deficient. In confirmed phthisis its presence seems to depend on whether or not there is accompanying fever. If this exist, i.e., if the disease is progressing, menstruation is absent; and, as is well known, its reappearance is looked upon usually as denoting to a greater or less extent an abatement of the mischief. When in slow, chronic cases menstruation is regular, the symptoms are frequently aggravated at the periods; and it is interesting to note that something similar to this occurs in other diseases. For example, a corresponding aggravation has been observed in insanity.² Associated with this it is, however, noteworthy to remark, that in the acute continued fevers—typhus, relapsing typhoid, and scarlet—menstruation usually occurs, and is frequently profuse. Fever, therefore, in itself can hardly be looked upon as the cause leading to its absence in phthisis. I am indebted to Dr Wood, of the Edinburgh Fever Hospital, for drawing my attention to the observation, that relapses in typhoid fever are frequently to be associated with menstruation. For instance, he informs me that if menstruation occurs within two or three days after the end of the third week, a relapse is very likely to occur; and I have had the opportunity of making similar observations in erysipelas. In the erysipelas ward of the Royal Infirmary, I had under my care a young woman who developed ten successive attacks of erysipelas in as many months. Each attack left a slight glandular swelling in the right submaxillary and parotid regions; and it was observed that two or three days before a menstrual period these became slightly more enlarged and rather painful, after which a relapse of erysipelas occurred. My idea of the explanation of such cases is,

¹ Ziemssen's *Cyclopædia*, vol. v. p. 494.

² Clouston, *ibid.*, p. 474.

that corresponding with each premenstrual period there is increased tissue metamorphosis, scarcely amounting, as a rule, to fever, but leading to increased resorption. Any morbid material which may at the time be in the organism—as, for example, the typhoid germs in the intestine, or the erysipelas germ in the enlarged glands—will then stand a better chance of being taken up again by the organism. The apprehension of such considerations to menstruation, when it does occur in chronic phthisis, is obvious. Not only is it likely to aggravate the general symptoms, but we may suppose that at the periods the chance of resorption and consequent fever will be increased.

V.—NOTES ON THE POSITION OF THE FLUID IN CASES OF PLEURITIC EFFUSION.

By J. SYMINGTON, M.D., F.R.C.S.E., Lecturer on Anatomy, School of Medicine, Edinburgh.

(Read before the Medico-Chirurgical Society of Edinburgh, 6th January 1886.)

IF fluid be effused into any of the large serous cavities, such as the pleural, pericardial, or peritoneal, it is obvious that it must produce some alteration in the anatomical relations of the neighbouring structures. The situations in which the fluid will tend to collect, and the nature and extent of the displacement of the neighbouring organs, are points of considerable practical importance, both in diagnosis and in treatment. The subject does not, however, appear to have received the attention it deserves—indeed, in the majority of works on medical diagnosis it is almost entirely ignored. Last winter session Professor Grainger Stewart read a paper before this Society, “On a Case of Tapping of the Pericardium, and on Tapping of the Pleura in the Treatment of Cardiac Disease,” which gave rise to a very interesting discussion. I was, unfortunately, not able to be present on that occasion; but, after a perusal of the report of the meeting in our Transactions, I must say that this aspect of the subject appeared to me to have been somewhat overlooked. The information derived from the ordinary physical signs, and the usual mode of conducting a post-mortem examination, do not afford a complete and satisfactory knowledge of the position and relations of the fluid. In addition to the theoretical deductions that may be drawn from the application of well-established physical laws, important information may be derived from experimental injections of fluids that afterwards solidify, or by frozen sections in which one or more of the serous cavities are naturally distended with fluid. J. J. Peyrot,¹ Powell,²

¹ *Etude expérimentale et clinique sur le thorax des pleurétiques et sur la Pleurotomie*. Paris, 1876.

² “On Some Effects of Lung Elasticity in Health and Disease,” *Med. and Chirurg. Transactions of London*, vol. lix.

Garland,¹ Pirogoff,² and others have contributed to our knowledge of this subject, yet there are several points that still require elucidation.

Garland suspended dogs by the head, and injected into their pleural cavities plaster of Paris and cocoa butter, so as to obtain models of the shape of the pleuritic effusion. He found that the fluid collected between the diaphragm and the lungs, and the line of dulness, corresponding to the juncture of the fluid and the base of the lung, was S shaped, being nearly identical with that described by Calvin Ellis in the human subject.

So far as I am aware, the only author who has published drawings of sections of the bodies of patients who have died with fluid in one or more of their serous cavities is Pirogoff; but, while some of his plates are very valuable, many of them are too rough and diagrammatic to be of much use.

The results obtained by the injection of such substances as plaster of Paris or cocoa butter into normal empty serous cavities are obviously liable to various sources of fallacy. The material is poured into the cavity much more rapidly than in an ordinary effusion, and the fluid is of a different specific gravity. These circumstances may have an important influence upon the position assumed by the neighbouring organs.

Frozen sections, in cases of natural effusion, are likely to give much more reliable results; but, except in places where bodies are plentiful and the climate a cold one, its practical application is attended with considerable difficulty.

In the course of an investigation into the topographical anatomy of the child, I have made frozen sections of the bodies of several subjects in which the pleural cavities were found to contain some serous effusion. Although my specimens are few in number, and were not prepared with the object of determining the position and relations of the pleuritic fluid, I have ventured to bring them before this Society, in the hope that they may direct attention to these questions.

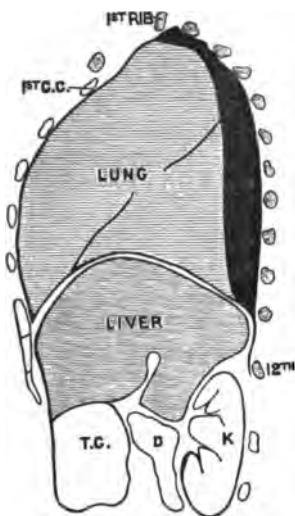
In all the cases to which I shall refer the entire body was frozen, the subject having been previously placed on its back in a horizontal position. In two instances the body was divided by several sagittal sections; in the others the cuts were made in a horizontal plane. The sections were traced, embedded in plaster of Paris, and hardened in spirit.

The woodcut is from a drawing of a sagittal section, one inch to the right of the mesial plane, of a male child aged about six years. The exposed viscera—lung, liver, kidney, supra-renal capsule, etc.—were apparently healthy. The pleuritic fluid had collected behind the lung so as to separate its posterior border from the

¹ "Some Experiments on the Curved Line of Dulness in Pleuritic Effusion," *Boston Med. and Surgical Journal*, 1874.

² *Anatome Topographica*. Petropoli, 1859.

ribs for about five-eighths of an inch; but the lung was in close contact with the anterior chest-wall and diaphragm. The fluid did not present a horizontal level along the whole extent of the pleural cavity. Thus at the upper part of the chest the fluid arched forwards above the apex of the lung, while below the fluid had not penetrated into the supplemental space, so that from the eleventh to a little below the twelfth rib, a distance of about an inch, the costal and diaphragmatic pleuræ were in close contact. A similar section was made to the left of the mesial plane, and the left pleural cavity was found partly obliterated by adhesions.



In another subject, that of a girl aged 13 years, sagittal sections were made 2 inches to the right and left of the mesial plane, and both pleural cavities were found to contain serous effusion. On the right side the fluid behind the lung was about 1 inch in depth, but from the tenth to the twelfth rib the costal and diaphragmatic pleuræ were in contact. The length of the supplemental pleural cavity was $2\frac{1}{2}$ inches. On the left side the position of the fluid was similar to that on the right, except that the supplemental pleural cavity was rather shorter. In horizontal sections of the thorax, such as I show in this drawing, the fluid presented a semilunar form. It was deepest behind the posterior border of the lung, but extended forwards a short distance on each side of the lung.

These sections confirm the general opinion that fluid in the pleural cavity tends to gravitate into the most dependent position, and the lung to float upon it. They support the views of Garland that the fluid does not maintain a horizontal level for all positions of the body, but that the shape of the fluid is modified by the lung. The lung is not altered in shape, although reduced in size. This is shown by the way in which the fluid extended forwards above the apex of the lung in the sagittal sections, and its semilunar form in the horizontal ones.

The non-extension of the fluid into the supplemental pleural space is an interesting fact, and one that I have not found any reference to in medical works. It could not be determined by percussion, on account of the presence of the liver on the right side and the spleen and kidneys on the left. It is worthy of notice as showing the difficulties connected with the plan of opening into and draining a pleuritic fluid from the lower part of the pleural cavity.

I believe that the results obtained by these sections are in accordance with what one would be led to expect from a study of the physical problems connected with the accumulation of fluid in the pleural cavity. The parietal and visceral layers of the pleura are kept in close contact by atmospheric pressure, and the lungs are maintained in a distended condition by the same force. As fluid collects in the pleural cavity the lung collapses, and the fluid tends to gravitate into the lowest part, as its specific gravity is greater than that of the lung. All parts of the lung are not equally compressible, or, as Garland puts it, the "retractile force" is unequally distributed over the lung, so that the fluid does not maintain a uniformly horizontal level, but is modified by the lung.

If air be freely admitted into the pleural cavity the lung usually collapses to about a third of its normal size, and it is extremely probable that the lung is readily compressed even though the fluid be poured out under a very low pressure. We should, therefore, not expect that it would force its way into the supplemental pleural cavity, and displace such organs as the liver or spleen, until the lung had almost entirely collapsed. It is difficult to determine how soon it would do so in the erect position of the body; but I am inclined to believe that it would not open up the supplemental cavity until it had produced considerable compression of the lung.

VI.—CASES OF OVARIOTOMY.

By SKENE KEITH, M.B., F.R.C.S. Ed.

(Read before the Medico-Chirurgical Society of Edinburgh, 6th January 1886.)

FROM my first fifty cases of ovariectomy I have selected several, which present special points of interest, either in the diagnosis, in the steps of the operation, or in the after-treatment. Details of an ordinary case of ovariectomy are without interest and are only wearisome; they are usually written by gentlemen who appear to think like an American friend, who had come to the conclusion that all his cases were "mortal bad." The two fatal cases of the fifty, and those are usually the most interesting, though often least heard of, have already been published in the *British Medical Journal* of 31st October 1885.

Mrs M., age 38, the mother of eight children, said that she first noticed a lump in the abdomen a year ago. Since that time it had slowly increased in size. An ovarian tumour, with probably posterior adhesions, was diagnosed, and it was removed on 10th June 1881. It was my first ovariectomy. Instead of the cyst being free in front, as I had expected, I found that it was closely adherent to the peritoneum, which was greatly thickened and almost as firm as cartilage; and, in fact, every morsel of the tumour

was adherent to something—parietes, omentum, or bowel. After separating and tying with fine silk over thirty bleeding pieces of adhesion I reached the pedicle, which was tightly twisted, and apparently did not contain any bloodvessels. A single loop of silk was tied round it to make sure that it would not bleed. The abdominal cavity was well sponged and a glass drainage-tube, reaching from the lower angle of the wound to the bottom of Douglas's pouch, was left in position. The interior of the cyst was almost in a state of gangrene. The twisting of the pedicle gave the reason for this, and also for the slow growth of the tumour. Its weight was 8 pounds.

Some months after going home Mrs M. was delivered of a fully matured male child. While under treatment here it was not suspected that she was pregnant, as she had menstruated a few days before leaving home. No special care was taken during the operation to save the uterus, which was a good deal pushed about by the sponging. In this case the pedicle had become twisted without causing any disturbance. When speaking of twisting of the pedicle, of course the slight rotation, which is met with in many cases, is not meant, but only when there has been interference with the circulation through the pedicle to the tumour.

W. B., age 24, says that five years ago she had an attack of severe abdominal pain, lasting for two weeks, and then observed a tumour in the abdomen. There had been no menstrual flow for ten months before this time, and since then the periods have been very irregular, sometimes appearing every fortnight. The tumour has increased very slowly, and there have been two other attacks of pain resembling the first one, though the pain was not so severe. A round, firm tumour was felt extending to 2 inches above the umbilicus, adherent probably on the right side. In the pelvis the uterus was felt to be small, with the body lying backwards. The history, and abdominal examination, pointed unmistakably to the tumour being uterine, but the pelvic condition made it clear that the growth was ovarian. At the operation on 7th March 1883, the tumour was found to have extensive friable connexions to the omentum. There was no pedicle. It had been entirely twisted off, and its remains were seen adhering to the sac. Although the tumour had been growing for at least five years, its weight was only $8\frac{1}{2}$ lbs. This was a typical history of a twisted pedicle; an attack of horrible pain followed by slow increase in size of the growth. The uterus was retroverted, and the right ovary was healthy. Unfortunately the specimen was not preserved, several other cases of twisted pedicle having been met with about the same time.

Mrs M., age 34, had noticed a hard lump in the right side three months before she was sent to me. In June of last year there was a firm, irregular tumour in the right side, closely connected

with the side of the uterus. In a month it had grown considerably, and a distinct pedicle could be felt, reaching from it to the uterus. In August it was fully twice the size it had been in June, and she had become pregnant. As experience has shown that the worst kind of adhesion is often met with where there has been one or more pregnancies along with an ovarian tumour, and as there does not appear to be any risk of causing abortion where there is an early pregnancy of three or four months, the tumour was removed without delay. It was a dermoid, and there were only slight posterior adhesions.

Mrs K., age 23, was delivered of her third child ten weeks before operation. No distinct tumour was noticed for some weeks after the birth, though the abdomen was thought to be large. At the operation the whole anterior surface of the tumour was found to be adherent. Probably the peritoneum had become directly connected to the cyst wall, and the growth of the uterus had stretched this connexion to the length of an inch. This had allowed of free movement of the parietes over the tumour, leading one to suppose that there were no adhesions in front. Thirteen months after the operation Mrs K. introduced her fourth baby to the world.

Mrs K., age 43, first noticed a hard lump high up in the left side two years ago; she had never had much pain. Urine contained 1-8th albumen on boiling and adding nitric acid.

15th November 1884.—Operation in the forenoon. The omentum, composed mainly of bloodvessels and connective tissue, with little fat, was of great size, and almost made a sac for the tumour, which weighed 34 lbs. A large part of it had to be removed. The second ovary was enlarged and adherent to the pelvis, and was also taken away. The albumen disappeared at once after the operation.

Mrs B., age 50, was sent into the Infirmary late one evening; she was very breathless, and had to sit in a chair before the fire all night. There was little urine, containing a half albumen. The abdomen was immense, and there was tremendous cedema of the wall. There was evidently a large quantity of free fluid in the peritoneum whatever else there might be. The history was as follows:—Four years before, a tumour was noticed in the left side, and was allowed to increase in size for three years, when she consulted a herbalist, who prescribed a bottle and some pills, which were taken for six weeks. At the end of that time she was much reduced in size, but says that she was almost dead from the purging and diuresis. Ten years ago, says that she had an attack of inflammation of the left kidney, lasting nine weeks.

Next day I tapped the abdomen, removing 52 lbs. weight of viscid, yellow fluid. I had to put the trocar in well above the umbilicus to

make sure of its reaching the abdomen cavity on account of the œdema. Two tumours could then be felt—the larger on the right, the smaller on the left. In three days the abdomen began to refill, and the albumen had almost disappeared from the urine. Two non-adherent tumours were removed on 8th December 1884. Their weight, along with the free fluid, came to 89½ lbs.

Mrs W., age 26, came to Edinburgh in June 1884. A year before, she had consulted Dr Whiteford of Greenock about an enlargement of the abdomen. Dr Whiteford advised her to go to Edinburgh; but, as the tumour did not trouble her at all, she delayed for twelve months. At no time since the swelling was first noticed has there been any abdominal pain, nor has there been any change in the menstruation. On examination, the abdomen was found to be uniformly distended by a smooth multilocular tumour, reaching up to the ribs. In front there were no adhesions, but in the pelvis the condition was doubtful, as part of the tumour was to be felt jammed low down on the left side of the uterus. However, the uterus was fairly movable.

On the 28th June I made an incision down to the tumour and emptied the largest cyst. This did not permit me to draw the tumour through the wound. I took out the trocar, enlarged the opening in the cyst, and broke up with the hand numbers of small cysts. I was then enabled to draw forwards the tumour sufficiently to see that the whole of its upper surface was attached to small intestine. This determined me to attempt to turn the tumour out from below. It was, however, so closely connected to the uterus, and had so extensively opened up the folds of the left broad ligament, that nothing in the shape of a pedicle could be reached. There was nothing for it, then, but to separate the adherent bowel from above. This I did, and found that the whole posterior surface of the tumour was covered by it. When this separation was almost completed I came on a round, whitish cord, about the thickness of the fourth finger. This was fortunately not very closely adherent, and was easily detached from the tumour. It was the left ureter. The tumour was too intimately connected to the uterus to allow of its separation, so after detaching the left broad ligament as far as possible, I was able to put a large clamp almost below the tumour, including a considerable part of the uterus in its grasp. Bleeding points, on which forceps had been left, were now tied with fine silk or catgut, and as sponges from the pelvis, both behind and in front of the uterus, came up almost dry, no drainage-tube was put in. The wound was carefully closed round the stump, and gauze, soaked in a solution of carbolic acid and glycerine 1 to 8, packed round the clamp. The tissue beyond the clamp was covered by a saturated solution of perchloride of iron in glycerine, and iodoform freely sprinkled over the whole. The weight of the tumour was 16 lbs. Time taken to operation, 58 minutes.

In the afternoon and evening the patient was fairly well. Temp., $101^{\circ}4$; pulse, 120. At nine o'clock next morning she looked very ill. There was a total want of expression; the face looked thin and gray. The temp. was $102^{\circ}8$; pulse, 136; respiration, 40, shallow, with occasional sighing. She complained greatly of a feeling of tightness across the chest. The abdomen was very flat. There was no movement of intestines and no sickness.

11 o'clock.—Pulse, 140. An enema of beef juice with $\frac{3}{4}$ ss. whisky was given, and ordered to be repeated every two hours.

2 P.M.—Looked as if she would not live twelve hours. The flesh seemed simply to be flying, not only from off her face, but also from the whole body. Unfortunately Dr Keith was out of town. She constantly complained that she was about to faint, and stimulants did not seem to revive her. I ordered six grains quinine by enema.

At 6 o'clock the patient had been asleep for an hour, and flatus had passed freely. She looked slightly better. Temp., $101^{\circ}2$; pulse, 144. The feeling of tightness across the chest was gone, and the attacks of faintness did not come so often.

10 P.M.—Looked much better; temp., $101^{\circ}2$; pulse, 138; 31 oz. urine since operation. There was after this no cause for anxiety. On the eleventh day I removed the clamp, opened up the stump, and found that there was a cavity barely an inch in depth. This cavity I filled with iodoform, and it slowly cicatrized from the bottom. The patient went back to Greenock forty-four days after operation, and on examining the uterus it was hardly possible to believe that it had been fixed in the wound, it was so movable.

The one interesting point about this case is the marvellous condition of shock which came on sixteen hours after the operation. It appears to me that the sympathetic nervous system had been injured or interfered with in some way, as was shown by the total absence of all intestinal movement, the feeling of tightness across the chest, and also probably by the repeated attacks of faintness coming on at one time every few minutes. Even in the worst cases of septicæmia I have never seen anything approaching to the rapidity with which the flesh disappeared from off this patient's body, for it was not only the face, but also the legs and arms which became emaciated.

A. M., age 22, has been aware of the presence of a swelling in the abdomen for fourteen months. For nine months after this was first noticed she was much troubled with vomiting occurring after meals. This finally ceased when she gave up wearing her stays. She has never been a strong girl; has always cold feet and hands, and a few months ago the left ankle swelled. The urine appeared to be normal, sp. gr. 1022. There was one large single cyst, and the possibility of its being a cyst of the broad ligament was thought

of, but the history and pelvic examination was against this diagnosis.

At the operation, on 26th August 1884, the tumour was found to be a broad ligament cyst.

Second day, 6.30 P.M.—Patient complains of headache. Temp., $101^{\circ}6$; pulse, 120. 41 oz. urine up till now; 10 oz. passed containing a trace of albumen.

8.30.—Temp., 104° ; ice to head.

10.30.—Temp., $102^{\circ}8$; pulse, 128; resp., 30. Nutritive enemata to be given every two hours.

Third day, 2 A.M.—Urine, 6 oz.; albumen, 1-6th.

4.30.—Urine, 6 oz.; albumen, 1-5th, with a trace of blood.

1 P.M.—Urine, 6 oz.; albumen, 1-4th; slight increase in the amount of blood; temp., $101^{\circ}4$; pulse, 124.

9 P.M.—Temp., 103° ; pulse, 150. Skin hot and dry; no decrease in the amount of albumen; face puffy. A ninth of a grain of nitrate of pilocarpine was injected and the ice taken off. In fifteen minutes the whole body was bathed in perspiration. Albumen, 1-3rd.

At midnight the pulse was very weak at 144; temp., 101° .

Next morning patient was decidedly better. Temp., $100^{\circ}4$; pulse, 140. Less albumen; no blood. The pulse came below 100 on the eighth day. There was no albumen on the seventeenth, and patient went home on the twenty-ninth day.

After the operation a remark was made that the sponges, which had been soaked in a two per cent. solution of phenol, had been used very wet, and I believe that there must have been more of the acid left in the abdominal cavity than the kidneys could get rid of without irritation. This case is a strong argument in favour of tapping every case of broad ligament cyst. Had this been done, all risk, trouble, and anxiety would have been avoided.

Mrs K., age 46, had been aware of the presence of something in the abdomen for two or three years. This something had much the appearance of a cyst of the broad ligament, but in the pelvis a hard mass the size of a hen's egg was to be felt.

Operation was on 18th May 1885. After emptying the cyst, which was a parovarian one, I found that it could not be drawn out of the abdomen, as it had opened up the right broad ligament, extending outwards to the ilium and deep down into the pelvis. The solid part turned out to be a small dermoid ovarian tumour, and was connected to the posterior layer of the broad ligament, the anterior layer being in front of the cyst. I enucleated down to the bottom of the pelvis, getting lower than the level of the external os uteri; next I removed a large part of the much hypertrophied broad ligament, and along with it the ovarian tumour, and fixed the rest of the ligament outside in a clamp, stitching the wound closely round it.

For six days patient knew nothing. The temperature ranged from 102° to $104^{\circ}5$; pulse about 110. The clamp was taken off on the third day, and a drainage-tube passed into the pelvis. During the second week there was free suppuration from the right broad ligament.

Probably the best thing to have done with the broad ligament, instead of fixing it in a clamp, would have been to have ligatured its edge in five or six pieces, and allowed it to fall back into the abdomen. The objection to using the cautery was the possibility of oozing going on behind, making a thrombus and separating the cauterized edges.

This case also shows the advantage of tapping broad ligament tumours. Had it been a simple parovarian tumour uncomplicated by an ovarian one, the difficulty and risk would have been the same, while tapping would probably have resulted in cure.

J. S., age 21, was sent to the Infirmary from Aberdeenshire, with a rapidly growing ovarian tumour, which had only been noticed four months before.

The tumour was multilocular, and had raised the ribs and sternum. The whole abdominal wall was cedematous. The tumour was adherent in front; the pelvis was full of it, retroverting the uterus. Two cysts were emptied to give relief, and to allow the cedema to disappear. The specific gravity of the urine was 1026.

The operation was on the 16th June. After breaking down the tumour, I had to separate adhesion from the whole anterior surface, and as high up as the border of the liver. The peritoneum was very vascular, and the bleeding was stopped partly by ligatures and partly by pressure of sponges from within. A drainage-tube was left in, as there was considerable hæmorrhage going on, high up about the liver. There was a good deal of shock, and the pulse was 128 immediately after the operation. At night she looked and felt weak, and enemata were begun. The temperature rose to $103^{\circ}4$. By morning pulse was 145; temp., $101^{\circ}4$. She was very weak. She was told that she might have whisky as often as she asked for it, and in the next twenty-four hours had 22 oz., which was, perhaps, rather too much; however, she would have died without a large quantity. The patient did not get on very well, and on the fifth day, as the face was slightly puffy, the urine was examined and found to contain fully a half albumen. The amount of stimulants was reduced, the loins poulticed, and a large dose of salts given, which moved the bowels three times by night, with the result that the pulse fell from 136 to 118, and the temperature from 103° to $101^{\circ}8$.

On the seventh day I took out the drainage-tube to allow the girl to lie on her side, as there was unfortunately a slough on her back, the only misfortune of the kind which we had in Ward XIX. of the Infirmary in six years.

On the evening of the ninth day the temperature was $102^{\circ}8$; pulse, 120. Next day both were under 100, and after that there was no trouble. During those first nine days the temperature was taken every two or three hours, and only ten times was it under 101° . The pulse was nine times counted under 125 during the same time.

Where nothing has been said in this paper about the convalescence the patient did well. This has avoided, as much as possible, repetition and wearisome details.

VII.—INTRODUCTORY ADDRESS TO A COURSE OF LECTURES ON DISEASES OF THE TROPICS AND CLIMATOLOGY.

By ROBERT W. FELKIN, M.D. et Chir. (Marburg), F.R.S.E., L.R.C.P., L.R.C.S. Ed., F.R.G.S., Member of the Anthropological Societies of London and Berlin; Member of the Scottish and Berlin Geographical Societies; and of the Obstetrical Society of Edinburgh, etc.

GENTLEMEN,—Robert Burton, in his address to the reader of his *Anatomy of Melancholy*, made use of some words which I take the liberty of adapting to my present need.

"I presume thou wilt be very inquisitive to know what antic or personate actor this is, that so insolently intrudes upon this common theatre, whence he is, why he doth it, and what he hath to say. So, in some sort to give thee satisfaction, I will show a reason, both of the title and the subjects of my proposed discourse, lest any man by reason of it should be deceived, expecting a satire, some ridiculous treatise, or some prodigious tenet. Besides, it hath been always an ordinary custom for people to broach many absurd and insolent fictions under the name of lectures, to get themselves credit, and by that means the more to be respected."

I trust "'tis not so with me.

"'No Centaurs here, no Gorgons look to find,
My subject is of man and human kind.'"

The subject on which I propose to give you a short course of lectures is one which, so far as I am aware, is new to this School of Medicine, and from all I can learn it has not had definite attention paid to it in any other medical school in the United Kingdom.

I think that it is the duty of any man who commences a course of lectures to state at the outset his reasons for so doing; and when he proposes, as I do, a fresh departure, if I may so say, he should all the more explain why he wishes to bring forward a new subject, and to claim for it some attention from the already greatly overburdened medical student. He should also set forth his own

qualifications for the performance of the task he undertakes, although, of course, he must stand or fall, not on his own estimate of the importance of his subject, but on the way in which it is received by the profession at large, and by his students in particular.

Since 1876, medicine, climatology, and especially tropical diseases, have had my close attention, and a residence of some nine years abroad, part of which time I spent in the tropics, has, I trust, given me some right to claim more than a mere passing or superficial knowledge of my subject.

It is quite impossible for lecturers on other medical subjects, who are at present so overburdened with material, that they find it difficult to compress the information which they have to impart within reasonable limits, to give more than a hint here and there on the subjects in question; and although I am strongly of opinion that they have been much neglected in the past, it is impossible to lay the blame at any one's door.

Another reason which may carry some weight is that these lectures will add to the completeness of the curriculum of the Edinburgh Medical School, already the most exhaustive in the United Kingdom; and I take it to be the duty of all in any way connected with this School of Medicine to do all in their power to keep up the noble traditions of the past, and strive, however humbly to enhance the good name which it justly bears in all quarters of the globe.

On looking over Churchill's *Medical Directory* the other day, I found that of the 25,000 odd practitioners of medicine, no less than 4500, or about a sixth, come into close contact with tropical diseases abroad (not counting those who for a year or two before settling in practice at home seek to extend their knowledge of the world by serving as surgeons in the mercantile marine). When we bear this fact in mind, and recollect the numbers who year by year go forth from this school to the tropics, I am constrained to think that an effort should be made to provide them with practical information as to the climates they will encounter, and the special diseases they will be called upon to treat.

To illustrate my meaning, I will assume that one of you is entering the army or navy, or going out to a colonial appointment or practice. In point of fact you may be, as it were, suddenly dropped from the clouds into some tropical region. In whatever capacity you act you may be called upon at a moment's notice to solve problems which will be perfectly new to you, and to which your attention has never been specially directed. You will in all likelihood be far away from consultants, as well as from your teachers, and even from colleagues with whom you might confer, and be thrown absolutely upon your own resources. Nor will you probably possess a large library for reference; even should you have one as extensive as that of one of the colleges it would cost

much valuable time and no little trouble to search volume after volume for information only to be found in odd sentences scattered through the wide range of medical literature. And I can assure you from personal experience that much of the practical advice you will require you will not be able to find after all. A short incident will illustrate my point. Once in returning to England from the tropics I went on board a ship, whose surgeon had been educated in a University of high repute. He was gold medalist in several of his subjects, and had passed his final examination with first-class honours. He had been house-surgeon and house-physician in more than one hospital, and was on his first voyage as ship's surgeon. Some time before I went on board, he had received a passenger, apparently at the point of death, suffering from a malady he did not know, and he was at his wit's end how to treat her. That the temperature was high he knew; that food could not be retained he knew; but he had never seen such a case before, and his attention had never been directed to tropical diseases, but only to subjects required for the examiners. His treatment consisted in giving two grains of quinine every three hours, and iced brandy and soda water. He had given up the patient, and her death was hourly expected by all on board. A short time after my arrival he asked me to see her. The case was clear to me, having seen many of the kind before, and, as I expected, the appropriate treatment saved the patient's life. On her leaving the ship, the surgeon received a cheque for £100. I, too, received my fee; it was a kiss, but this, gentlemen, was from the young lady's father!

I feel it due to this doctor to balance the story I have just related by another. When I first went to the tropics I also was often at a loss to know what to do in many cases, although I had had much valuable advice before I went, and was therefore not absolutely ignorant of what lay before me. I remember once being in a small hut hundreds of miles away from any aid. A man lay on a rough litter, his life ebbing away, and I, having done all I could, was powerless to save him. An hour later I stood at an open grave, and, surrounded by a crowd of savages, I read the burial service. And then as I saw the earth mound rising over one who so lately had been in full vigour of life, my mind was filled by anxious thoughts; had all that was possible been done for him? Such questions crowded on the mind in its loneliness, and induced a feeling of almost crushing responsibility.

I would have you consider, gentlemen, what special subjects you must study beforehand to enable you to meet such difficulties, and to bring all the essential facts of strange and untried circumstances within your grasp when you go—

"To distant climes, a dreary scene,
Where half the convex world intrudes between;
Through torrid tracts with fainting steps to go
Where wild Altama murmurs to your woe.

Far different there from all that charmed before,
The various terrors of that horrid shore ;
Those blazing suns that dart a downward ray,
And fiercely shed intolerable day ;
Those matted woods where birds forget to sing,
But silent bats in silent clusters cling ;
Those poisonous fields with rank luxuriance crowned,
Where the dark scorpion scatters death around,
Where at each step the stranger fears to wake
The rattling terrors of the vengeful snake ;
Where crouching tigers wait their helpless prey,
And savage men more murderous still than they ;
Where oft in whirls the mad tornado flies
Mingling the ravaged landscape with the skies.
Far different these from every former scene,
The cooling brook, the grassy vested green,
The breezy covert of the warbling grove,
That only shelters thefts of harmless love."

Apart from all general medical study, it will be needful for you, in order to fit yourselves for a tropical sphere of practice, to make yourselves acquainted with various influences exerted by tropical climates on natives and on Europeans ; to gain some knowledge of meteorology ; to obtain general information on the manners, customs, and ideas of the people ; and above all, to study as far as possible the diseases of the country in which you intend to cast your lot.

True it is that in the medical profession, as in all others, a definite and somewhat limited training is necessary, but those who go abroad must possess more than the knowledge provided by the ordinary curriculum. Gentlemen, you must lay in store a reserve fund of common sense ; you must free your minds from the dominion of prejudice, of preconceived opinion, of early bias, of popular delusion.

We have had an excellent example of what I mean in the late Soudan campaign. Soldiers and sailors who had been trained to European warfare found themselves suddenly compelled to lay aside their usual tactics and to adapt themselves to other methods of carrying on war. Soldiers were transformed into boatmen ; sailors, although not "horse marines," were mounted on camels, and many such anomalies were inevitable. Although the soldiers showed great bravery, there is no doubt that the sailors proved themselves, as they always do, to be the most capable of adapting themselves to new and untried circumstances. Why was this the case ? Because their life on board ship is one which develops their common sense, makes them self-reliant, and enables them to turn their hand to anything.

I must now pass on to consider some of the elements which will affect your practice in the far off regions whither the fates may guide you.

I will first call your attention to some general remarks on acclimatization.

Acclimatization is the process, oftentimes slow, by which plants and animals become adapted to, and so retain health in countries having a different climate to those in which they are indigenous. This acclimatization may be in part effected by changes taking place in the individual or in the race, and in part by hereditary modification of constitution. Some authorities deny the possibility of real acclimatization, but allow that a person may accommodate himself for a certain time to almost any climate.

It is only during the last three centuries that information has been gradually collected upon which we can base any accurate knowledge of acclimatization. This is explained by the fact that until then far off lands were unknown to the civilized nations. I do not forget that when the Romans ruled the whole of the then known world, people were transferred from one climate to another, but these often compulsory emigrations took place almost entirely within the temperate zone.

The comparative physiology of different races will alone provide us with materials for the investigation of this subject, and it is in this direction that all doctors in foreign lands can lend extensive aid to science.

I may note in passing that some analogies may be found in the breeding of animals and in the cultivation of foreign plants. For instance, the South Down sheep could not exist in the bleak mountain tops where the smaller black-faced sheep thrive, although the latter do well on the Downs. A mixed breed of these two will be less hardy than the one, but more so than the other. Again, the Australian "Monkey-puzzle" (the *Araucaria imbricata*) flourished in Britain until the hard winter of 1860, when no less than nine-tenths of the species perished.

Now, if we look at the distribution of different races throughout the world we find that great changes have taken place in their location. The Esquimaux once lived in Asia, only some 40 degrees north of the line; now they inhabit the polar circle. The Bohemians or Gypsies are found nearly everywhere, while Jews and Maltese apparently thrive in every conceivable region.

If, however, we examine these facts more closely, we find that the Esquimaux did not arrive at their present hunting-grounds in a generation, much less in a few weeks, as emigrants and soldiers are compelled to do now, nor do we find that the Bohemians or Jews overspread the world rapidly. Far from it; they put out feelers, as it were, and only very gradually, year after year, generation after generation, did they advance from their native soil into the great unknown. Looking for a moment at other races, we observe that the British soon become acclimatized at the Cape, in Southern Australia, in New Zealand, in some parts of North America; not so in India. The French thrive in Nova Scotia and in Mauritius, but not in the north of Algeria, though in the southern provinces they do somewhat better. The Dutch, as we

know to our cost, fare exceedingly well at the Cape, not so in the Malay Peninsula, where they have experienced fearful mortality. I may instance Madagascar as having a climate that has proved most unfavourable to all Europeans. These facts show us that different races bear in varying degrees the influence of climate, and each possesses more or less power of resisting such influence.

It will not do to divide mankind simply into two classes, white and black, as some have done in treating this subject. This is far too sweeping a generalization, and we must examine it much more closely if we are to come to definite and right conclusions. We must endeavour to prove with certainty in each separate case what power of resistance is possessed by any given national constitution, in order to decide whether it may successfully acclimatize itself in a hot country and permanently colonize there. In white races there seems to be a marked gradation in their susceptibility to climatic influence, and there is a very decided difference between the Aryan and Semitic races in this respect. The Arabs and the Jews, as I previously mentioned, are both unequalled in their power of adapting themselves to a new environment; but even between them a difference exists, in so far as the Jews appear to continue definitely capable of reproduction, although they marry amongst themselves, while the Arabs often suffer from degeneration, which is only retarded by the introduction of new blood. They take wives from the races among whom they settle, and thereby continue to exist. The race is altered and transformed into a mixed race, which eventually possesses very little in common with the original stock. For example, lapse of time and difference of climate have exerted such an influence upon the Turks that they now present physical and mental characteristics widely different from those possessed by their forefathers in the 13th century.

Spaniards, more especially those from the south of Spain, withstand the Algerian climate much better than the purer Aryan races, and can we not trace this to the fact that they inherit a large proportion of Semitic blood from the old Moorish invaders? As the Spanish language possesses numerous Arabic elements, so also do the people, and to this may be largely attributed the power of resistance to climatic influence which they manifest. The same remarks apply to all those inhabitants of southern Europe upon whom similar influences have been exerted, and the contrast between them and the northern European nations in this respect is very apparent.

Race itself does not always provide us with a definite clue to the capability of acclimatization, for in India the Hindoo population, notwithstanding its Aryan origin, has thriven under unfavourable circumstances, and even in malarial districts, thus presenting a striking contrast to the English (also an Aryan race), of whose intolerance of the Indian climate we are only too well aware.

Not only is the physical constitution of a race to be taken into account, but temperament also plays a not unimportant part in the possibility of prolonged existence in a given region, and it is a quality which requires considerable length of time to be modified by new conditions of life. For instance, the Indian of Brazil is dull and sullen, the Negro is vivacious and gay, although in the same climate. Hereditary temperament and capacity of mind characterize variously all races, and differences of intellectual and moral power are well-marked factors in the possibility of acclimatization.

We have now for a moment to consider those climatic influences which prevent acclimatization. Hippocrates in his treatise, 'περὶ ἀέρων, υδάτων, τόπων,' first struck the keynote of the investigation of what characteristics in man and his environment affect his well-being. Celsus also gave some attention to the influence upon man in health and disease of climate and diet; but the first writers who really developed the subject in a scientific manner were Finke and Schnurrer in 1792. Since then much has been written upon geographical pathology and medical geography, culminating in Dr Hirsch's painstaking book on *Geographical and Historical Pathology*, an English translation of which has been published by the New Sydenham Society; and in the remarkable climatological contributions of the *Encyclopédie des Sciences Médicales*.

In France, England, and America an extensive literature has accumulated, the study of which will at once show how important it is to investigate both the distribution of disease and the influence which climate has upon it, and also how disease affects races differently under varying climatic conditions.

In a recent discussion on the subject of acclimatization in Berlin, Professor Virchow called attention to the fact that in most works on this subject, two questions really very distinct have been confused. These questions are (1) "How long can any single individual with precautions and care live in any particular climate?" and (2) "What races can thrive and colonize in any particular climate?" As yet medical authorities have given no very definite answer to these questions, and probably no satisfactory answer will be given until a more thorough investigation has served to elucidate those morbid influences with which a man or a race has to contend in various isothermic zones. To-day I shall content myself with calling your attention to the tropical and sub-tropical zones in reference to this subject, as it is with these regions that my lectures principally have to do, and I shall confine my observations to (1), the Climate of the Tropics; and (2), Malaria, as being the most important morbid agent produced by climatic conditions.

In considering the temperature of the tropics, we must not only take into account the mean annual temperature, but also the

duration of a given high temperature and the rapid diurnal variation of heat. Temperature is popularly thought to depend entirely on the distance of a given place north or south of the equator and from the sea, but if you examine the breadth and position of the tropical and sub-tropical zones upon the map before you, it will appear that variations occur which at first sight may be surprising. The boundaries of these zones do not run straight across the map, following different and definite parallels of latitude. Look, for instance, at the tropical zone. It ends at Cape Verd, latitude 15° N., longitude 16° W. of Greenwich, whereas in Hindostan it extends as far as latitude 22° N., longitude 75° E. of Greenwich, on account of the position of that country. South of the equator it extends on the west coast of South America as far as the 4th degree of latitude, but to $22\frac{1}{4}^{\circ}$ S. at the 50th degree of longitude E. of Greenwich, showing a difference of $18\frac{1}{4}$ degrees. Again, if you look at the south limit of the south temperate zone, you see that it extends in a fairly straight line across the map at about latitude 62° S. of the equator, whereas its northern boundary has a very irregular configuration. The north temperate zone has similar characteristics as you observe on the map, which shows these zones somewhat diagrammatically, of course, but with fairly accurate colouring.

The tropical zone itself has a mean annual temperature for all months of the year of over 70° F., the two sub-tropical zones have the same temperature during part of the year varying from four to eleven months. The temperate zones, on the other hand, which have a mean temperature of 32° F. in the coldest months and 68° F. in the hottest, are subdivided, one zone being coloured dark green having a hot summer and a cold winter, the other, light green, having one to four months hot weather, and eight to eleven months of winter.

The map shows almost at a glance those regions in which it will be difficult if not impossible for a European to become acclimatized; or where, although he may exist for a longer or shorter time, he will be incapable of any great manual labour.

Looking at Africa, you notice that with the exception of Cape Colony and a very small portion of the north-west, the whole of the continent is in the tropical and sub-tropical zones. Mexico, Central America, and nearly all of South America, occupy the same zones, as do also Arabia, Persia, India, Burmah, a great part of Australia, and islands too numerous to mention. If Europeans attempt to colonize in these vast areas they almost invariably die out. We find a striking exemplification of this in the West Indies; and even in Cuba, which is brought forward by some authorities as a proof to the contrary, the apparent increase in the white population is more than accounted for by immigration.

It is noteworthy that the permanent decrease of the white population in these islands dates from 1833 when slavery was

abolished; and Dowding attributes it to this cause, showing that as overseers or occupiers of the soil Europeans are able to withstand climate more successfully than when they are compelled to undertake actual manual labour. In Cuba, slavery will not entirely cease till 1888, which is therefore another reason for its favourable contrast with the surrounding islands. The white man's incapacity for manual labour is an important question throughout the tropics; it has necessitated the employment of Chinese labour in America, of coolies in Mauritius and Java, and of kidnapped natives from the South Sea islands in Australia.

You will notice that I said Europeans were *almost* incapable of colonizing in the tropics. I said this advisedly, for there are places occupying a high altitude, and some islands whose climate is so modified by the surrounding water and by their exposure to winds, that they almost possess a temperate climate. For instance, in the Vindhya Hills in India, the French have colonized successfully; and, theoretically, there is no reason why Europeans should not colonize and thrive in some of the highlands of Central Africa. This I personally believe to be quite possible, if railway communication were provided to carry them rapidly across the dangerous belt of malaria on the coast.

Rainfall necessarily influences climate. The heat between 5° N. and 5° S. of the equator is greatly modified by the abundance of the rainfall. The climate is thereby rendered more enervating and unhealthy, for moist heat is more trying to the constitution than dry. We find, on the contrary, the greatest heat and driest atmosphere, not as is commonly supposed at the equator, but forming a belt some 3½ degrees in width at a distance of some 20 degrees north and south of the line. Within this zone rain seldom or never falls, and a very hot but comparatively healthy climate is met with. In the Sahara, rain ceases at about 16° N. and begins again at about 28° N., this being the maximum breadth of the rainless belt.

The influence which winds exert upon climate is very great; in India, for instance, the wet and dry seasons depend chiefly upon the monsoons. Mountain ranges also modify climate, for they attract moisture, and snow or rain results from the coolness of the atmosphere around their summits, and although clouds and fogs, rain and snow, are always to be met with in high altitudes, the climate is nevertheless healthy and invigorating. I shall enter more fully into all these subjects in a subsequent lecture; what I have said must suffice for the present.

(To be continued.)

Part Second.

REVIEWS.

Handbook of Diseases of the Skin. Edited by H. v. ZIEMSEN, M.D., Professor of Clinical Medicine in Munich. New York: William Wood & Co. London: Sampson Low, Marston, Searle, & Rivington: 1885.

THIS splendid volume, whose advent has been long and anxiously expected, has been presented free of all expense to the subscribers, numbering some thousands, to Ziemssen's *Cyclopædia*, by the publishers of the American edition, Messrs William Wood & Co. There is no precedent for this generosity. It speaks well both for the success of the *Cyclopædia* and for the independence and the enterprise of the publishers. The work itself marks an era in the history of dermatology which corresponds in some measure to that fixed by the appearance of Hebra's and Kaposi's *Diseases of the Skin*. There is this difference, however—fourteen years elapsed between the date of the first and that of the fifth volume of Hebra, while the whole of V. Ziemssen's work, in its English dress, is given to the world at once. In executing the translation, which is most creditably performed, a certain degree of condensation has been made use of. We have compared the English version with the original frequently throughout, and are bound to say that the sense, even where the compression has been considerable, has been well brought out. The omissions have been chiefly those "extensions of the predicate or of the subject," which in the form of illustration or redundancy have been employed to make the meaning clearer. The result has sometimes been that pretty close attention to every word in a paragraph has been necessary fully to follow the train of reasoning. This makes the book, more particularly when we listen to Unna or Auspitz, rather stiff reading; and it may be said at once that it is not a work for the tyro in medicine. A fair, nay, a pretty accurate acquaintance with the skin and its diseases is necessary for its proper comprehension. It will not, therefore, displace any of the existing manuals or textbooks, but can be appealed to from them as to a higher authority. Thus Unna, in describing the anatomy and development of the skin, treats of the hair in a much more complex manner than has been heretofore attempted; nor does Auspitz endeavour to reconcile his classification with that of any other author. Again, the article on syphilis deals with the essential nature of the disease, the connexion of this with micro-organisms, its mode of communication, and how it diffuses itself throughout the system, but not at all with its clinical features, and only incidentally with its treat-

ment. Throughout, the work is a philosophic one, but the practical side is only here and there in abeyance. It will be found well developed in many parts, and valuable hints reveal themselves unexpectedly, like the diamonds in the blue clay, to a sifting examination.

It is not a treatise which can be analyzed or subjected to minute criticism in the space at our disposal. All that can be accomplished in that direction is to select certain subjects here and there to show how these are dealt with, and then to let the inference be drawn how the mass is treated.

Among the numerous questions of interest which Unna sets himself to answer in his article already alluded to, with which the volume opens, is the one of hair change in man. One is prepared for much that is a little ingenious and novel, and the picture presented to us of this phenomenon is both. The usual statement is, that after a longer or shorter time in the life of a hair its term of existence comes to an end, and it is cast off, to be replaced by a new hair growing either from the same papilla or from a new one in its neighbourhood. The procedure by which this is accomplished is, according to Unna, a much more intricate one. It consists in an ascent of the papillary hair in the follicle after its separation from the papilla, though only as far as the middle region. No sooner does it cease to be attached to the papilla than it loses the dimple-like depression at its root, and its fibres spread out and assume that brush-like aspect so familiar when one examines the root of a hair which has naturally fallen. The papilla and the lower third of the follicle become atrophied, and the hair having ascended as far as the part of the follicle immediately below the sebaceous gland, remains firmly attached to the prickle cell layer, and grows out from this situation. Unna calls this part of the altered follicle the hair bed, and the hair so growing a bed-hair. After an indefinite period an epithelial process is sent downwards from the bed-hair, and this process develops into a young papillary hair, which in process of growth pushes out the bed-hair and takes its place. There are, then, two kinds of hair in man—papillary hair, which possesses a medulla, and bed-hairs, which, so long as they grow from the hair bed, contain in that part no medulla. This same process occurs in the fœtus before or soon after birth. The presence of long, thick, and dark hair at birth on the infant's head is due to the persistence of bed-hairs, which should have fallen off before. "The hair change of the new-born is therefore a true interchange between two entirely distinct types of hair. It would be extremely interesting to investigate the question whether the hair change of puberty, of adolescence, and of different forms of alopecia, should also be referred to a similar change of type, or are to be otherwise explained."

Unna believes that the special function of the sebaceous glands is to lubricate the hair, while the sudoriferous glands take an

active part in supplying oleaginous material to the epidermis; and in this view V. Ziemssen coincides in his short but complete article on the physiology of the skin. To the sweat or coil glands, as he prefers to call them, he also assigns the production of the subcutaneous cushion of fat. The connexion between the two has often been noticed; but the causal relationship has, so far as we are aware, been first traced by Unna. He traces this from the development of both in the fœtus, from the coincident change which takes place in their chemical constitution, and from their topographical relationship.

The effect of varnishing the surface in man is placed in a different light than that in which it is usually viewed by V. Ziemssen. The case of the boy who died after being gilded over to take part in the installation of Pope Leo X. is shown not to be a strong one as evidence, since we are not told what symptoms occurred, and whether the substances used in gilding were not themselves poisonous. He concludes his remarks by saying, that it appears that in man, or at least in adults, it is not dangerous to cover the cutaneous surface with an impermeable coating. This opinion comes in very opportunely, when collodion, traumaticine, and gelatine preparations are so largely employed in skin diseases.

The article on the General Pathology and Therapeutics of the Skin, by Auspitz, is one in which the author has brought out the complex kinship which exists among cutaneous disorders. We have not read a better account anywhere than is there contained of the resemblances and differences between psoriasis and lichen ruber. It all but justifies the name lichen-psoriasis, which Mr Hutchinson has given the latter. Auspitz does not allude to the pathology of lichen ruber according to Robinson, but he clearly explains the production of a lichen ruber acuminatus, and planus, respectively, leaving no doubt of their essential identity. Much of the article had already appeared in Auspitz's *System der Haut Krankheiten*; but as this has not been translated, it was a sealed book to many in this country, while the sections on etiology and treatment, containing valuable hints, are entirely new. Regret has sometimes been expressed that the continuous bath recommended by Hebra for severe burns and chronic diffuse diseases of the skin had not been introduced here. This will be much lessened when we learn that Auspitz has been "convinced, from prolonged personal observation, that in the majority of cases, neither a favourable influence upon the cure of chronic skin diseases, nor upon the diminution of pain or pruritus, or upon the course of extensive suppurative processes and burns, is effected thereby." In parasitic diseases, especially scabies, he has seen good results, and no inconveniences from a storax soap.

The Superficial Inflammations of the Skin are allotted to Veiel of Cannstatt. These are those due to thermal, mechanical, or chemical influences, and catarrh of the skin—eczema. The article

on Eczema is well executed, though brevity has been perhaps rather too much strained after. It would seem as if the dry form of eczema of the palms and soles, by no means uncommon here, were much less so in South Germany. Veiel appears only to have seen two cases. The same rarity is encountered in Denmark. Do habits, racial characteristics, or climate influence this? Tar occupies a place in the treatment which it has not attained to, either in this country or in America. The value of chrysarobin ointment in dispelling obstinate thickenings and infiltrations in chronic eczema is specially insisted on. Fissures are healed most readily by soft soap, which is painted into them twice a day. Pyrogallic acid and naphthol are also more in favour with Veiel than so far they have been in Britain. Lupus erythematosus is also included in the same class. Veiel does not rank it with the neoplasms, "notwithstanding its focus-like aggregation of cells in the corium, which brings it into close anatomical relation with lupus vulgaris." It is true its real nature is still obscure, yet as bearing on its close connexion with lupus vulgaris, we lately saw a case in which, on the face and ears, the two diseases were quite inextricably blended. In small and circumscribed patches iodide of mercury ointment, 1 in 5 to 15, is recommended, a single application being made after the patch has been cleansed from fat and scales, permitted to remain till vesicles form, then wiped off, and the vesicles allowed to dry and fall off. No scars are left, and the cure may thus be rapid.

Erysipelas which with other deep spreading inflammations is handled ably by Geber, will always be regarded with special interest in Edinburgh, where Bell first proved the value of muriate of iron in arresting its progress, and Lister and his followers displaced it from its position as an hospital scourge. The account given by Geber is thoughtful and complete. He adduces strong proof against the belief in an idiopathic form, that in all cases it requires a point of entrance, produced in a traumatic manner. When this cannot be discovered, he believes that the slight injury—erosion—remains concealed. He is of opinion that the micrococci which have been associated with it are not indispensable requisites for its production. "According to his conception, a deleterious substance is produced under specially irritating influences, either *in loco*, or it reaches the inflammatory spot from the outside, and produces erysipelas in predisposed individuals." With one statement we are not disposed to agree, "that we are generally unable to check the progress of the disease, and the large number of remedies, under which he includes chloride of iron, are of problematical value." There can be no doubt in the minds of those who have administered muriate of iron as *freely* and as *frequently* as it should be, that it exerts a most powerful effect in checking erysipelas. Geber adheres to an expectant, symptomatic treatment, which, so far as it goes, is rational.

When the individual portions of such a work are entrusted to different hands, one is prepared to meet with considerable variety, not only in the manner, but also in the ability with which the subjects are treated. The chronic deep spreading inflammations, scleroderma, and elephantiasis, by Schwimmer, are models of accurate condensation, and the same may be said of ichthyosis and some other anomalies of the epidermis described by Lesser. Schwimmer includes, as many authors now do, morphea under scleroderma. While admitting that the process may undergo retrogression if it has not reached its acme, he thinks this curative procedure rarely occurs spontaneously, but by the aid of the appropriate remedies which stimulate the process of absorption, and are capable of rendering the rigid skin flexible. Especially does he recommend the constant current. It is particularly in cases which have been recognised early that a favourable result may ensue. We cannot so favourably report on the article on Anomalies of the Epidermis contributed by Weyl, though some portions are much superior to others, and injury has been done by the excision of paragraphs in translating from the German. Thus, while the clinical features of psoriasis are well expressed, the treatment does not include those more recent, elegant, and effective methods which have come into use within the last few years. The illustration of the histology of psoriasis is a copy of one which appeared in this Journal in 1880. Lichen planus is imperfectly described; the acute form is not even alluded to, the verrucose only very cursorily, and not by name; and from the translation one would not suppose that it was identical with lichen ruber. Lichen circinatus, by implication, includes lichen scrofulosorum. Pityriasis rubra and rosea are separately described, and a contagious form of acute pemphigus in children, which Weyl believes to be, in many cases, what is called impetigo contagiosa. The whole article conveys the impression that it is more a compilation than the outcome of well considered and mature observation; and if we are to judge from this, the teaching at Berlin must be very different from that in Vienna. From this we turn to the thoroughly masterly treatise on the chronic infectious diseases of the skin by Neisser. What are included under this term will be best understood from the statement "that those comprise a group of affections the origin and course of which are dependent upon organized infectious matters which reproduce themselves within the body." The idea of infectious, therefore, does not here coincide with contagious diseases in general. Thus there are presented to us tuberculosis of the skin, including lupus vulgaris, leprosy, syphilis, mycosis fungoides, frambcesia, glanders, actinomycosis, trachoma, rhinoscleroma. The interest of these centres round lupus and syphilis, but Neisser's studies of leprosy have given him a peculiar grasp of the whole class, and the essay must be regarded as one of the most interesting, as it is one of the

most profound, in the volume. Whether we agree with him as to a proved or implied bacterial origin of these diseases or not, there can be no question that many obscure points are here presented in a clearer light. This is particularly the case with regard to syphilis, but much the same may be said with respect to tuberculosis. The treatment which is preferred in lupus is that by mild caustics, as pyrogallic acid, and antiseptics, as iodoform, in preference to the methods by erosion and scarification. In this direction the influence of the doctrines of Besnier is perceptible.

An excellent *résumé* of our knowledge of the neuroses of the skin is contributed by Schwimmer. The only cutaneous neurosis described by Hebra-Kaposi was pruritus, though it was indicated that the class might be largely augmented. This has taken place, and in addition erythema multiforme, and nodosum, still—we think erroneously—separated, urticaria, prurigo, herpes, and malum perforans pedis, with some others, are so classified. The only practical note we would make is, that Schwimmer has found much benefit in chronic urticaria from a nightly pill of 1-150th grain of atropia, when other remedies have failed. Michelson gives a very complete account of the anomalies in the growth and colour of the hair. Among other remedies he recommends chloride of sodium as a local application in alopecia, associated with pityriasis. This will comfort those visitors to the seaside who find their loss of hair temporarily increased. He accepts as fully proved the occurrence of sudden blanching of the hair, though unable to explain it.

Lesser, under Anomalies in the Colour of the Skin, fully describes those peculiar reddish-blue macules to which attention has of late been directed, as occurring in connexion with the pediculus pubis, and which, though caused by the parasite, are produced only in certain individuals. The pigmentary syphilide he shows to be really a leucoderma. The original pathological element is the occurrence of the whitish spots, while the increased pigmentation is secondary and comparable to the effects of chrysarobin in psoriasis. In acne rosacea and sycosis, which are among the diseases assigned to E. and T. Veiel, a 2 per cent. alcoholic solution of pyrogallic acid is recommended as a very successful mode of treatment in special cases. The affections of the nails receive full justice from Geber, though we think the value of the article would have been enhanced by some reference to the observations of Hutchinson. Racial peculiarities are specially manifested in parasitic diseases, though habits and environment exert an influence on their localization and nature; hence the articles by Weyl and Geber on these are coloured by those circumstances. Thus it is strange to read of pityriasis versicolor being of almost daily occurrence in the inguinal region, and of the extreme rarity of tinea tonsurans of the hairy scalp in Germany. This portion, therefore, loses some of its value for us, but, apart from these distinctive features, will be found full and exhaustive.

The volume is brought to a close with an account or description of the new formations in the skin, by Schwimmer, Babes, and Geber, whose names are a sufficient guarantee for the manner in which these are treated. A connexion of a causal nature between affections of the liver and xanthoma is rejected, after a careful analysis of the cases. *Molluscum contagiosum* is described under the name of *epithelioma molluscum*, its origin in the rete, and not from the sebaceous glands, being accepted. This curious disease seems fated to have names attached to it liable to occasion confusion.

The press corrections in the volume are by no means so perfect as they should be; and here and there the translation might have been better worded, though in only one or two passages have we found that the original was misconstrued. As only subscribers to the entire *Cyclopædia* can obtain this, it is thus beyond the reach of many members of the profession; and we can only hope that the liberal publishers may devise some means whereby a work on skin diseases, which will not be superseded during the next decade, might be to be had separately.

Atlas of Venereal Diseases. By P. H. MACLAREN, M.D., F.R.C.S.E.
Edinburgh: Young J. Pentland.

WE have received the two first fasciculi of this Atlas, and are well pleased with it. The plates of cases of cario-necrosis and ulcerating gummata are especially good. The letterpress description of the plates is clear, condensed, and accurate.

A Complete Pronouncing Medical Dictionary: embracing the Terminology of Medicine and the Kindred Sciences, with their Signification, Etymology, and Pronunciation. With an Appendix comprising an Explanation of the Latin Terms and Phrases occurring in Medicine, Anatomy, Pharmacy, etc.; together with the Necessary Directions for Writing Latin Prescriptions, etc. By JOSEPH THOMAS, M.D., LL.D.
Philadelphia: J. B. Lippincott Company: 1886.

THE above title is a comprehensive one, and in some respects is fully borne out by the character of the volume. So far as it goes, the book is a monument of patient industry, careful research, and praiseworthy accuracy, while a laudable attempt is made to grapple with the difficulties which beset the pronunciation of scientific terms derived from the Latin, and reduce them to laws of rule and order. The book is a portly octavo of 844 double-columned pages; and had the author not been sorely handicapped

by the conditions attending the practice of medicine in America, space might have been available for much which is deficient, and which would have rendered the work more valuable as a book of reference on this side of the Atlantic.

In his preface the author tells us that "in the United States a classical education is not made an indispensable condition for conferring a medical degree;" and "the fact that multitudes of our young men commence the study of medicine without any previous acquaintance with Latin or Greek renders it important that the elementary works intended for the use of students should be adapted to meet this exigency." Herein lies the kernel of most of the shortcomings of this otherwise excellent volume. A goodly portion of it is appropriated to the explanation and elucidation of elementary matters; and when the author, in a valuable appendix, is compelled to expiscate the simplest rules of Latin Grammar, and explain the A B C of pharmacy in a manner suited to the comprehension of the veriest tyro in the science, the value of the Dictionary as a work of reference is curtailed, and its comprehensiveness is necessarily circumscribed. We are not in favour of esotery in connexion with any science, far less in that of medicine. Attempts to invest it with the veil of secrecy are apt to degenerate into charlatanism; but we are not disposed to throw the portals of our science open to a lower qualification, or render the "open sesame" easier of accomplishment than what is implied by the preliminary examinations requisite on the part of those who would cross the threshold of our Esculapian fanes. Medicine, with its cognate sciences, like the unexorcised demon of Eastern fable, is steadily growing into colossal dimensions, and demands a proportionate service from its votaries; and no attempt short of an honest endeavour to fulfil the terms of servitude will meet with countenance or favour.

One marked feature of the work is the inequality of its articles, so far as their relative importance is concerned. When we find the article "Pathogenesis" occupying a space equal to 32 pages of an ordinary octavo volume, one might suppose the intention had been to form a collection of medical treatises; and when we examine the space devoted to Spirits, Syrups, Pills, Tinctures, and Unguents, the work might be taken for a handbook of *Materia Medica*. Elementary though the work be, it is not lack of space which excludes the notice of such diseases as Aleppo Button, Ainhum, and Chloroma; and while, for example, the unimportant articles "Abstract," "General," and "Park," have upwards of 30 lines devoted to their elucidation, we search in vain for Basilyst, Colpitis, or Sponge-grafting. While, generally speaking, the Dictionary is abreast of the later additions to the American Pharmacopœia, there are omissions which detract from the value of the work. Not only genera, but also all the species of plants which are officinal, should have a place in a work of this kind. A

reference to "Ailanthus" will illustrate this, while the Ajugæ are altogether wanting. The omissions referred to by no means exhaust the catalogue: they form but a few taken at random. Newer discoveries, such as Antipyrin, Kairin, etc., are not to be expected; but indeed no dictionary of the future will be in any sense complete without embracing the newest discoveries in medicine and therapeutics. We trust another edition of the work under review will witness a rectification of these blemishes. In other respects the book is well got up, and is a splendid specimen of typographical skill and correctness.

Morality: An Essay on some Points thereof (addressed to Young Men). By MAURICE CHARLES HIME, M.A., LL.D., Head Master of Royal College, Londonderry. Eighth Edition, revised and enlarged, pp. 175. London: J. & A. Churchill.

It may be truly said that one of the chief features of modern medicine is the rise and progress of that branch of it which deals with prevention. "Prevention is better than cure" is being looked upon not as a common-place proverb, but as a guiding principle in medical science. This is specially true of all that concerns the relations of micro-organisms to disease, and of what may be classed under the head of Hygiene; but another and no less important section is for various reasons still beyond the professional pale. Few classes of diseases involve such varied and extensive suffering as those included under the title of Venereal Diseases, and yet the fountain source of them all—the due control of the sexual instinct—has not yet been deemed especially worthy of professional care. We have keen enough discussions on the merits of certain restrictive regulations. Could we but go a step further back, we would strike more surely at the root of the evil, and render such discussions unnecessary. The above essay by Mr Hime comes in to guard the portal of health against the great class of diseases we have indicated, and many others besides of a less definite character. We therefore gladly welcome it from a professional point of view no less than for its general moral excellence.

The book is written in excellent taste, although the subject is fairly and openly treated. The seductive fallacies which mislead so many are reviewed and exposed, and the aids to a successful fight on virtue's side are clearly and ably discussed. Frequent quotations from the best English authors are used to back up the arguments of the text. Altogether we can heartily recommend this book. If placed in the hands of young men about to enter on their life of personal responsibility, it is calculated to be of the greatest service.

A Schoolmaster's Retrospect of Eighteen and a half Years in an Irish School. By MAURICE C. HIME, M.A., LL.D., Head Master of Foyle College, Londonderry. Pp. 134. London: Simpkin, Marshall, & Co., Stationers' Hall Court. Dublin: Sullivan Brothers, 27 Marlborough Street.

THIS book enters into the discussion of many matters which are of interest in education. The abolition of corporal punishment is strongly advocated, and latterly the writer had tried the experiment of doing away with punishment of every form. He had not had time to fully test the results when the book was printed.

Those who wish to qualify for higher masterships are strongly recommended to take a degree in Medicine. The book is pleasantly written, and is worthy of perusal.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH,

SESSION LXV.—MEETING IV.

Wednesday, 6th January 1886.—Dr DUNCAN, *Vice-President, in the Chair.*

I. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members of the Society:—Charles Kennedy, M.D.; William Gayton, M.D., M.R.C.P. Edin., London; Reginald E. Horsley, M.B., C.M.

II. EXHIBITION OF PATIENT.

Dr Philip showed a case of URTICARIA, presenting one or two characters of interest. The eruption had developed acutely, with no very evident cause, the patient going to bed well and finding, when he awoke in the morning, the characteristic wheals widely distributed. They were present on the back of the thighs, on the buttocks, and the lower part of the trunk posteriorly. They had remained out for ten or twelve days, during which time the patient suffered acutely, and sleep was impossible. In addition, towards the upper limit of the eruption on the left side, a number of vesicles and bullæ had developed, so that in part the case presented an ecthymatous appearance, and suggested the possibility either of the conditions being one of erythema exsudativum multiforme, or of the existence of a concurrent herpes zoster. The history, general distribution, and other appearances were opposed to either of these views. It must be regarded as an urticaria, assuming locally those intense characters. These,

coupled with its remarkable persistency, would, in the more formal terminology of some of the schools, lead to its classification as *Urticaria perstans vesiculosa et bullosa*.

III. EXHIBITION OF INSTRUMENTS.

1. *Dr Nasmyth*, Cowdenbeath, showed a NEW FORM OF BOX-SPLINT. It was intended to overcome the difficulty of treating oblique fracture of the leg. This was done by means of a rack at the foot, to which plaster straps were attached for application below the seat of fracture, and counter-extension was made by straps attached to the top of the splint. He had found it also useful in the treatment of compound fracture of the leg.

2. *Dr Foulis* showed (a.) A DEVICE whereby the instrument for aseptic catheterization, shown at two previous meetings of the Society, could be applied to any catheter. (b.) An INSTRUMENT, made on the same principle as his aseptic catheter, for washing out the stomach. (c.) An ASEPTIC CANULA, intended to obviate the objection to the ordinary aspirator when plugging occurred. A stream of an antiseptic fluid was made to pass through the canula, while a blunt trocar was inserted to clear it.

Mr Skene Keith was interested in the canula, but thought it was too complicated to come into general use. The apparatus used by his father for many years for tapping the abdomen was much simpler. A trocar and canula was inserted, the trocar withdrawn, and the fluid rushed out. The tube from the aspirating bottle was at once attached to it. It occasionally happened that the canula became blocked, but this was overcome by simply taking off the tube and clearing the canula with the trocar. He had never seen a bad result follow.

Dr Foulis said *Dr Keith* might do so in the case of the abdomen, but he defied any man to do it to the pleura without introducing sepsis.

Dr Shand said that in the days before antiseptics were heard of he did exactly what *Dr Foulis* objected to. He tapped the pleura on several occasions, using a new well-made and clean stomach pump, and the results were most satisfactory.

Dr Troup said that he had several times tapped the pleura without using those special precautions that *Dr Foulis* insisted on. In some of the cases the fluid was purulent. The results were highly satisfactory, and the patients were alive to this day. It had also happened to him one morning on riding into a village to be called to see a child just big enough to reach up to a kitchen dresser, open it, and abstract some laudanum, of which it swallowed one ounce. He had a catheter with him which he passed into the stomach and washed it out, inverting the child to get the siphon action. The case did well.

Mr Joseph Bell hoped *Dr Foulis* would not call the aspirator he

had on the table Dieulafoy's, if he intended to publish an account of his instrument. The aspirator of Dieulafoy had not the objection to which Dr Foulis alluded when plugging occurred, but by using its back action any plug was easily and safely dislodged.

IV. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr A. G. Miller* showed the parts from a HIP-JOINT AMPUTATION, in which transfusion had been employed, which appeared at page 721 of this Journal.

Mr Chiene regarded this method of transfusion as one of the most important of the later improvements in surgery. It was evident that in proportion to the body weight the patient had more blood in him after than before the operation. He had it in fluid form, which was of very great importance. It was fluid that many of these patients wanted, there being usually so little fluid circulating that the heart could not contract upon it. He had been interested to observe in the course of some experiments he had made that it was impossible to abstract all the blood from the body of an animal by ordinary bleeding. If an animal were bled to death a large quantity of blood was always left in the abdominal veins which cannot be abstracted. He wished to ask if the phosphate of soda solution was aseptic.

Dr Duncan said that the solution was made with boiled distilled water. It was, however, impossible to use the spray, as the mixture of carbolic coagulated the blood.

Dr Foulis asked if this solution of phosphate of soda, or of some analogous salt, might not be used in medicine to prevent the coagulation which occurred as a result of pneumonic disease. It was a wonderful fact in physiology that the blood did not coagulate in the bloodvessels until it flowed into the area of the injured tissue. Could the mechanical plugging of bloodvessels, caused by coagulation of the blood, be prevented by adding to the blood a certain percentage of phosphate of soda or some other salt?

2. *Dr Symington* exhibited a specimen of DISEASE OF THE VESICULÆ SEMINALES AND TESTICLES. The epididymis on the right side was much enlarged, and on section was seen to be converted into a caseous looking mass. The corresponding vas deferens showed no traces of disease. The pelvis was divided by a coronal section, passing through the bladder, vesiculæ seminales, and second part of rectum. The right vesiculæ seminalis was enlarged, so that on examination per rectum it could have been felt as a rounded mass. On section its ducts were found greatly distended, and filled with caseous material. The left testicle and its epididymis appeared to be healthy, but the cavity of the tunica vaginalis was partly obliterated by adhesions. The left vesicula seminalis was atrophied, and its cavity obliterated.

V. ORIGINAL COMMUNICATIONS.

1. *Mr Skene Keith* read his paper on SEVERAL CASES OF OVARIOTOMY, PRESENTING SPECIAL POINTS OF INTEREST, which appears at page 837 of this Journal.

Prof. Simpson congratulated Mr Keith on the success of his first fifty cases of ovariectomy. Several of the cases he had brought before them were of great interest, particularly those in which torsion of the pedicle had occurred. Most of them illustrated a great fertility of resource in the operating, and in the management of the cases afterwards. They were all proud of the reputation Dr Thomas Keith had won by his success in this operation, and were delighted to see the son following his father's footsteps. The paper which Mr Skene Keith had read—and read just a little too rapidly for the members of the Society to get the full benefit of it—seemed to him a very valuable one. Perhaps its value might have been increased if Mr Keith had offered some observations as to the peculiar conditions that had come under his notice. For example, he should like to have heard a little more regarding the torsion of the pedicle. Mr Lawson Tait's view, that this was due to the action of the rectum, would explain the last case he (Prof. Simpson) had met with. In that case a right-sided ovarian tumour had had its pedicle twisted as if its left border had been turned forward and to the right, which was the direction the twist would take under the pressure of the sigmoid flexure of the colon and rectum on the back of the tumour.

Mr A. G. Miller said the general impression he had derived from hearing those cases was a confirmation of the opinion he had long held that the peritoneal cavity stood a good deal of rough handling, and that abdominal surgery was not a good test of antiseptics. If the abdominal surgeon were to treat a stump in the same way as a peritoneum was usually treated, the stump would go wrong to a dead certainty. Sepsis would assuredly follow. Another thing that impressed him was how near to death many of the patients seemed to be, and yet they recovered.

Dr Black said he had seen several ovariectomies, by different operators, before the days of antiseptics. They were almost all fatal, though every precaution appeared to have been taken, excepting that the operations were done in a crowded theatre, before large audiences, which probably had a bad effect on the patient.

Mr Cathcart asked if the explanation of the power of introducing septic organisms with impunity was not dependent on the rapidity with which the peritoneum absorbed? If a certain limited quantity of putrid material were injected into the peritoneal cavity it did no harm, but if a quantity which exceeds the absorbing power of the area on which it lay were injected, then septic peritonitis followed.

Dr Duncan asked if it were not the case that if an aseptic fluid

were injected into the peritoneum in a sufficiently large quantity, bacteria would develop and septic peritonitis result? He thought that was one of Burdon-Sanderson's experiments.

Mr Keith, in reply, said he could not answer Prof. Simpson's question about the twisting of the pedicle, because in the anxiety to get the operation done quickly there was not time to notice such a detail. To be accurate it would be necessary to take a note at the moment of observation, because on one occasion when they had remarked the direction in which the twisting had taken place those present found themselves after the operation unable to agree upon it, some saying it was from right to left, others that it was from left to right. He rather thought Mr Miller had forgotten that to get into the peritoneal cavity one had to make a wound which was occasionally a pretty extensive one, and had to bear as rough a handling as the peritoneal cavity. If septicæmia did not occur through the peritoneum it might through this wound. Mr Miller was right about the nearness to death of many of the cases. The first day or two were the worst for the patients. After that they were almost always advancing to convalescence if they were to get better.

2. *Dr Symington* read some NOTES ON THE POSITION OF THE FLUID IN CASES OF PLEURITIC EFFUSION, which appears at page 834 of this Journal.

Dr James Ritchie said that Dr Symington's communication was a valuable as well as an interesting one. He had demonstrated that the upper level of pleuritic effusions was not a horizontal plane. Certain clinical features had led them to suspect this. On examining the back of the thorax in cases of effusion the breath sounds were heard at a lower level, near the spinal column, than further from it, showing the presence of less fluid in the former situation. At the upper level of the fluid it was found that the changes in both percussion and auscultation were not abrupt, and the ægophonic vocal resonance met with in such cases was explained on the theory that there was a thinner layer of fluid at the upper limit than further down. Dr Symington had concluded that the fluid did not reach the lowest part of the pleural cavity, but that the two surfaces of the pleura were there in contact, but Dr Ritchie doubted if Dr Symington's illustrations were a correct representation of the condition during life. In his case the lung was collapsed, or at least in a state of more complete expiration than would ever be the case during life, and the diaphragmatic and costal pleuræ would never during life be in contact to so great an extent.

Dr Shand had had a case under his care in which during a course of phthisis the patient died of an intercurrent attack of pleuro-pneumonia. The post-mortem examination showed the lung exactly as it appeared in one of Dr Symington's illustrations. As confirmatory of what he said regarding the position of the fluid, he mentioned a case in which he had tapped two ribs below the axilla.

The fluid flowed splendidly. Nothing could be more successful. There floated something on the top which, on examining more closely, he found to be a piece of adventitious membrane.

Dr W. Russell said that the absence of fluid from the supplementary pleural sinus was not in accordance with the observations which had been made by others. The fluid had been described as gravitating and finding its way there in the first place. In some of the recorded observations the experiments were made on living animals, and it was found that the position of the fluid depended on the position in which the animal operated on had been kept, and when in a suitable position the fluid always gravitated into this supplementary pleural sinus. It would be a matter of importance if *Dr Symington* extended his investigations to the position of the heart during the presence of fluid in the pericardium. He had been present and had taken part in the discussion on this matter recently, and he was not quite satisfied as to the most desirable position at which to aspirate the pericardium.

Mr Cathcart asked if the sinuous line which the top of the fluid took could be explained by capillarity? Had the thickness of the column of fluid between the chest-wall and the lung and the height to which it rose any connexion?

Mr Miller asked whether empyematous fluid sank lower than ordinary pleuritic effusion? That was his opinion, and hence the reason for tapping lower in a case of empyema.

Dr Symington, in reply to *Dr Ritchie's* objection, said the lungs in his specimen and illustrations were not in a state of collapse, but in the condition of expiration in which the chest always was after death. So long as the pleural cavity was not opened the lung did not collapse further. He quite agreed that some of his points were already known, but there had been neglect of them in all the ordinary text-books, and none of the recent graduates to whom he had spoken appeared to know anything about them. The point which had not been referred to before was the non-extension of the fluid into the supplementary pleural cavity. This was a matter well worth studying. He brought it forward mainly to direct attention to it, because physicians had apparently neglected it.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XLVII.—MEETING II.

Wednesday, 9th December 1885.—*Dr HALLIDAY CROOM, President, in the Chair.*

(Continued from page 772.)

Mr A. W. Hare thanked the Society for the opportunity they had accorded him of being present at this meeting, and expressed the great interest with which he had listened to *Dr Barbour's* thoughtful and well-weighed address in opening the evening's

discussion. As a surgeon, it was not altogether without precedent that he should take a deep interest in the subject matter of the discussion, since the late Sir James Simpson had repeatedly drawn attention to the close analogy which exists between a recent stump and the post-partum uterus. In both there is a wound to be healed, and both are exposed to a series of dangers which, if not absolutely identical in the two cases, are at least very closely related. In his paper, Dr Barbour had most carefully restrained himself from drawing any wider inferences as to the relation of micro-organisms to puerperal septicæmia than the most rigid deductive process would allow. This was a very wise precaution to observe in opening a debate on a question such as the one now under discussion. At the same time, it was a question whether from the premises adduced in the case of puerperal septicæmia one might not take a step further than this, and adopt as a working hypothesis, in relation to this disease, what was now an assured basis of discussion in the case of other forms of septicæmia—that a micro-organism is either directly or indirectly the exciting cause of the condition. In this respect there was no reason for separating puerperal septicæmia from the varieties of septicæmia which come under the charge of the surgeon, and in what followed the speaker would not discuss the fact of that relation, but the circumstances by which it was conditioned, and its essential nature in a variety of cases. In considering the condition of the uterus during involution, one was led to inquire why it should specially offer a nidus for the development of infective materials. Some light had been thrown on this point by the researches of Fischel,¹ who finds that apart altogether from the fatty changes which occur in the organ at this period, there is a very large production of peptone amongst the muscular fibres. This peptone formation is only an expression of the rapid reduction and removal of tissue elements in a soluble form, for as the tissues ordinarily are insoluble, and can only increase by taking up soluble material from the nutrient media which percolate through them, so also can they only diminish in bulk by becoming in greater or less degree soluble, which permits of their removal in solution in these same media. Accordingly, we find insoluble myosin breaking down into soluble peptones, which are thrown off in part by the kidneys, giving rise to the peptonuria of the early puerperium,—in part also, in all probability, by the secretion of mammary glands. We have then in the puerperal uterus a soluble material prepared by the exigencies of the physiological condition of involution, and in this material any micro-organisms which gain access to it can multiply the more rapidly from the fact that it is in a soluble condition, and can be immediately applied without further preparation to the requirements of such organisms. In addition to this, the tissues are depressed by recent excessive activity, and are less resistant than usual to infective invasion.

¹ *Archiv. für Gynäkologie*, Bd. xxiv. 1 and 3.

One other point which must of necessity be looked to is the possibility of retention of membranes or blood-clots in the uterine cavity. In septicæmia one must recognise two distinct types of pathological action, which have been clearly differentiated by Erichsen. This duality of type has been further accentuated and insisted on by Professor Chiene in his course of lectures at the University. In the one type there is an invasion of living tissues by micro-organisms and their elaborated products. In the other type there is an entrance of elaborated products or ptomaines only, the organisms producing them being confined to dead matter retained in contact with an absorbent surface. The organisms in the former case are pathogenic, and the morbid process they give rise to is a septic infection. The organisms in the latter case are saprogenic, and the morbid process they set up is a septic poisoning. The former develop in living tissues, the latter exclusively in dead material. Taking up the first type, that of septic infection, one finds comprised under it, as Professor Simpson has suggested, a congeries of different processes. Several facts point to this conclusion. *Firstly*, the infection may be traced to erysipelatoid, scarlatinoid, diphtheritic, or other distinct forms of virus. *Secondly*, the clinical features of these infective conditions point to the same fact, for there are at least three types to be differentiated clinically, viz.:—*a*. Rapid toxic paralysis; *b*. A specific fever; *c*. A pyæmic type. In the first of these there is rapid infection with so virulent an agent that death occurs within 24 hours, and the tissue reaction observed *post-mortem* is *nil*, or but a slight trace of splenic congestion. In the second form a specific fever runs its course, selecting either mucous or serous surfaces as its battleground, giving rise respectively to gastro-intestinal inflammations and metastatic affections of the serous cavities, and in these cases there is usually enormous splenic enlargement. In the third form the disease pursues a much more deliberate course. Secondary infective foci are formed in various localities by transference of irritative material from the primary focus, and a true pyæmia results. Etiologically and clinically considered, these cases point strongly to a polymorphism of the infective virus. *Thirdly*, a similar conclusion is come to in experimental pathology, and this may be said to strengthen the foregoing conclusion by a legitimate use of analogy. In the house mouse two kinds of septicæmia may be produced artificially—one due to a fine bacillus, this disease being peculiar to this animal only; another due to a thicker organism, which produces a fatal septicæmia in the rabbit.¹ In the mouse these diseases are identical from the clinical point of view. Leaving here the type of the septic infections with its various subdivisions, the type of septic poisoning must claim consideration. This is due to the group of organisms called saprogenic, which live only upon dead material, and give rise to changes in it, ordinarily described as putrefactive changes. The dead material is in this

¹ Koch, *Traumatic Infective Diseases*, New Syd. Soc.

case outside the body, external at least to living tissue, but placed in contact with a living surface, through which absorption may occur. A poisonous product is set free in the decomposing matter, and is absorbed by the neighbouring living tissue, and the condition of septic poisoning, or "sapræmia," is occasioned. The organism here cannot develop in living tissues,¹ but though it is excluded, its elaborated products obtain access and produce distinct poisoning effects. These products consist of an alkaloid, "sepsin," first obtained by Bergmann,² and of similar bodies described as ptomaines, the pharmacological effects of which are now attracting considerable attention.³ In this type of septicæmia the symptoms are very definite, and display a tendency to become from time to time exacerbated, each exacerbation corresponding to a repetition of the absorption which takes place at the seat of decomposition. This type offers the greatest amenity for treatment, for by the use of antiseptic solutions the sepsin manufactory may be destroyed, and the poison already absorbed into the system may be rapidly eliminated. Far otherwise is it in the type of septic infections: when once established, their ejection by therapeutic means seems almost an impossibility, and they must run their course, opposed only by the vital activity of the tissues. Here prophylaxis is the chief object, by securing as high a vitality as possible in the patient, and by rigorously excluding any chance of infective contamination in operative manipulations. A new sphere is opened in these cases for the activity of pharmacologists, in providing us with specific substances whose action is physiologically antagonistic to that of these infective organisms and the products they elaborate. That such specifics may be found and employed with success is sufficiently indicated by the recent researches of Duclaux,⁴ who has found for certain species of organisms antagonistic chemical agents, the minutest trace of which completely stops their vegetative activity. It is along this line that the next great advance must be made.

Dr Peter Young regarded the subject under discussion as one of the highest importance. At one time, before the introduction of antiseptics, this disease was the bugbear of midwifery, and one might say it is so still in spite of all the precautions taken with the means at our disposal. It is interesting to note the change of view that has been taken by obstetricians as to the cause of the condition. He recollects well when Scanzoni regarded with contempt the idea that an endemic poison was the source of the infection, and denied the evidence that any organism outside the mother had anything to do with it. He (*Dr Young*) admits that the cause is a poison either formed in the patient or introduced by the obstetrician. But he believes, moreover, that we have

¹ Hiller, *Die Lehre von der Fäulniss*, 1879.

² *Das putride Gift und putride Intoxication*, 1866.

³ Cp. Brieger, *Die Ptomaine*, 1885.

⁴ Recent papers in *Comptes rendus de l'Académie des Sciences*.

symptoms of septicæmia produced where there is no solution of continuity. He mentioned the case of a girl of six, who after a meal of sausages had all the symptoms of puerperal fever. There was first well-marked peritonitis. Three days after the local symptoms almost disappeared, but general symptoms ensued, with a high temperature and pulse, etc. These symptoms showed that a certain amount of the poisonous material had entered the blood from the bowel. After running a definite course the symptoms, under purgatives, etc., gradually abated, and the patient recovered. The blood of a puerpera is liable to absorb and provide a suitable nidus for the poison, and her general conditions favour its development. He has always been of opinion that the *materies morbi*, whether *autogenetic* or *heterogenetic*, was not to be looked for in a germ, but in an albuminoid substance produced in the blood in a state of catalysis. This view, whether correct or not, does not alter our practice. It is of importance to keep all infection away from the patient. We are not, however, to regard stinking lochia an indication for washing out the vagina. Our practice of this operation must be determined by constitutional conditions. It is, moreover, of the utmost importance that we attend to the condition of the alimentary canal. We know how foetid are abscesses which are opened near this canal, and we can only account for such a characteristic by the fact that poisons find their way readily from the canal into such abscesses, and give rise to putrefactive changes which are not common in other abscesses. It is thus of importance to attend to the evacuation of the bowel, so as to prevent, as far as possible, infection from this source.

Dr Leith Napier said—We must congratulate Dr Barbour on having brought under the notice of the Society so very important a subject for debate. The subject is one of ever increasing interest, and recent work has but added to our desire for more labour and more knowledge therefrom. Despite our as yet but imperfect knowledge, the existence of micro-organisms in puerperal diseases is not a thing of yesterday; it dates back twenty years. Mayrhofer, in 1865, discovered vibriones in the lochia of sick puerperal women. Rindfleisch, in 1866, described softened metastatic deposits, consisting of vibriones, as existing in the muscular tissues in cases of pyæmia and puerperal fever; beyond the term "vibriones" he mentioned no specific distinction as to the variety of these organisms. Recklinghausen, in 1871, described certain bodies found in the smallest renal veins, in the glomeruli, in the pulmonary alveoli, on post-mortem examination of patients dying from typhus, pyæmia, etc., as *micrococci*. He, with Waldeyer, showed that these existed, in puerperal cases, not only in the lochia, but on the surface of puerperal wounds, in the uterine lymphatics, in the infiltrations of connective tissues, and in exudations in serous cavities. Braidwood and Vacher's papers, published some years ago, showed that healthy lochia contained micro-organisms "free

from infection, but capable of producing it." Micrococci occur in many affections, *e.g.*, erysipelas, scarlatina, hospital gangrene, variola, vaccinia, measles, whooping cough, dysentery, endocarditis, osteo-myelitis, acute abscesses, puerperal fevers, and in typhus (according to Mott), and diphtheria. Bacilli occur in typhoid, typhus, pyæmia, diphtheria, etc., etc. Orth states that micrococci are present in the blood in many diseases, particularly in septic diseases, puerperal fevers, diphtheria, etc.; but he expressly states that they are by no means constantly found. Eberth has borne similar testimony. Koch regards the pathogenic bacteria as of distinct species, saying the spherical never pass into rod-shaped forms. On the other hand, Schenk and Ray Lankester affirm that micrococci have been observed to develop into bacilli, and these again into spirillæ. Klein remarks that true micrococci never elongate to form rods, but mentions that certain septic (non-pathogenic) organisms may be changed into pathogenic forms. Pasteur (to whom bacteriology owes so much) holds that the lochia of a puerpera in good condition contain few or no micro-organisms; but should there be a threatening of puerperal disorder, there is abundance of microbes. He, by the examination of the lochia from a patient, predicted an attack of fever before the slightest clinical symptoms were manifest. The form of organisms he describes as the principal, but not the only one, he has named *Chaplet en grains*. "Puerperal fever," says Pasteur, "has no special microbe, but several forms." Artwing mentions that different forms of puerperal septicæmia are due to one living poison—one micro-organism—but that this has not been proved to be special to the puerperal state. Lormer observes that in puerperal fever, scarlatina, erysipelas, etc., chain-like micrococci are found, which have not as yet been shown to possess individual differences. We must remember also that micro-organisms may exist in disease, as in pneumonia of swine fever, yet have nothing to do with the malady, which according to Klein is due to infection through the air. And again, in many conditions mixed forms are found; by some these are regarded as accessory bacteria—anyhow, chain-like and rod-like forms are often found in the same individual. According to Professor Ogston, septicæmia (septic infection) is most frequently due to absorption of the ptomaines poured into the blood from some local deposit of micrococci, but more especially in puerperal cases as the result of development of micrococci or bacilli in the blood. Mr Hare and Dr Underhill have referred to what seems to me the most important distinctions possible from a clinical and pathological standpoint, viz.,—the differences between septic poisoning and septic infection. In the former (which I may remark is more common in surgical than obstetrical practice) we have the origin from endo-sepsis; the condition formerly described as sapræmia; the condition in which we find the ordinary bacteria of putrefaction or decay, the *bacterium termo*. In the latter the mischief may be either from

exo-sepsis or auto-sepsis, and it is this variety which is connected with swarms of micrococci or bacilli of pathogenic nature. I do not think we can as yet decide whether the one may or may not pass into the other; clinically we might reasonably expect so, but if so, if the generation of the pathogenic from the non-pathogenic conditions is possible, it has not yet been proved; and the patient suffering from endo-sepsis is a most likely soil for the implantation of exo-septic mischief. The pyæmic organisms differ from the septic, inasmuch that they grow on the bloodvessels causing their sides to adhere, form colonies, thromboses or plugs, and multiple abscesses. In erysipelas the organisms are found either primarily or from an early period in the lymphatics. I am sure that, in their wide experience, Professor Simpson and Dr Angus Macdonald have seen cases of erysipelas existing in puerperæ without puerperal septicæmia. Dr Macdonald's case mentioned showed that, until, from spreading, the disease reached the genitals, no septic infection took place. While erysipelas is a grave condition sometimes, yet in many cases it exists in puerperal or pregnant women simply as a local affection; personally I have recorded such cases. There are certain theories, apart from micro-organisms, we must not lose sight of: the changed state of the blood (which I need not enter on), the excess of lactic acid from muscular exertion during labour, the deficiency of phosphates from expenditure of nervous energy, and the formation of the peptones alluded to by Dr Hare. For the establishment of puerperal septicæmia, we need a special condition of the system, blood, etc., a special poison (?) and its entrance at the proper time. As many of the Fellows are aware, I have devoted much thought to the subject of metria. In my paper published in 1875, and more fully in 1880 in the *Obstetrical Journal*, I postulated that "puerperal fever exists at least in one form as a disease *sui generis*;" or, with more recent and exact definition, I might now say that puerperal septicæmia or puerperal septic infection, not puerperal septic poisoning or endo-sepsis, exists in one form as a distinct disease; and this because, and here I give the reasons as briefly as possible,—(1) at times large numbers of puerperal women are affected with fever, (2) this is not solely dependent on septic material conveyed manually, (3) nor on erysipelas, (4) nor is it ordinary pyæmia or septicæmia, (5) it is contagious, (6) like typhus, etc., complications of varying nature may appear, (7) it is most virulent in hospitals, (8) and distinct from its complications, peritonitis, metritis, etc., (9) it is infectious to fœtus in utero or newly-born child, (10) exists as an epizootic when epidemic or endemic, (11) post-mortem appearances are not more inconstant than in other general diseases, (12) in course of epidemic may become pandemic—new comers more liable to infection than residents. I have also urged that there is a special state of the nervous system predisposing to septicæmia. We may ask what influence have micro-

organisms on septicæmia? do they cause it or follow it? In some cases, infectious, the one; in others, poisoning, the other probably. If there is any special microbe, then there is a special disease. True, such has not yet been shown to exist, yet manifestly different diseases, such as I have already mentioned, cannot in reason be supposed to be due to but one variety of micro-organism. There are doubtless great difficulties in estimating the differences. When we see certain observers using powers of 400, others 700, others 1600, others 2600, it is not easy to rely on all the measurements as distinctive. Colour shows some difference. Osteo-myelitis has a yellow, acute abscess a white microbe; the former disease cannot be produced from inoculation by the specific micrococcus unless there is diseased bone somewhere. The selective affinities of microbes are as yet but imperfectly understood. Why Koch's field mice were proof against the septic infection of the house mice? why Koch, Davaine, and Pasteur, have severally got different results with their various artificially prepared sepsines? are questions we cannot as yet answer. How do the microbes gain admittance in puerpera? By the skin possibly, by the lungs very probably, by the vagina without doubt. Every puerperal woman, aye, almost every pregnant woman after the sixth or seventh month, is theoretically a suitable and likely nidus for the development of septic mischief. That Nature has as much to do with the prevention of such mischief as Art, in the great majority of cases, is indisputable. Every puerperal woman is a suitable case for ordinary putrefactive micro-organisms to form—do they always do so? In many cases, yes; in others it is more than doubtful. Now, if owing to certain influences these might become modified into pathogenic formations, the whole question would be easily solved. Is this so? Present opinion is adverse to such a belief, yet certain putrefactive changes doubtless modify the resistant power against an inpouring of exo- or auto-septic microbes. The ante-partum existence of puerperal fever or septicæmia cannot be explained away, as has been attempted by certain disciples of the germ-after-theory school. Fordyce Barker, Campbell (in the Edinburgh epidemic of 1822), and I, have mentioned several cases, and many others exist in literature. Well, we have certain cases of distinct septic infection, or septic ærial transference, or exo-sepsis, or doubtfully, very doubtfully, purely auto-sepsis; these cases are rapidly fatal, and leave little or no post-mortem explanatory condition. Can these cases be due to the invasion of microbes? or can they be explained by spontaneous genesis of the micro-organisms? The alkaloid of Bergmann referred to by Dr Hare, and described as a yellow crystal-like body, has received little notice; how much it deserves I will not presume to say. Sir James Simpson's time-honoured comparison of the post-partum uterus to a stump left after amputation was an imperfect comparison: the one is a physiological, the other a pathological condition; the one is natural, the other artificial. The septicæmia,

closely allied to pyæmia, may be almost regarded as similar. In pyæmia the deposit of organisms then forming zooglea or colonies, and the resulting thrombotic conditions, are well-linked parts of but one chain. The second heading of Dr Barbour's valuable paper is of unquestioned importance. I regret that I, having already occupied so much time, can only touch it in outline. Antiseptics may be divided into two classes—those used as local or external, or in- and external, and those given internally by the mouth, rectum, or skin. We have to consider, first, the routine treatment in all cases. After delivery I generally advise the application of a napkin scorched at the fire, frequently changed, and the bathing of the external parts with whisky and water, half and half, twice or thrice daily for the first two or three days. When the patient can procure them, I strongly advise sanitary towels in preference to the ordinary, often imperfectly purified, napkin. Hartmann's wood-wool antiseptic towels are excellent. A piece of Gamgee tissue, which has been soaked in perchloride of mercury solution and rapidly dried, makes a capital substitute. This is very cheap, and of course is burned after use. Iodoform gauze is good, but the smell is objectionable, and any gauze is irritating. Regarding the routine use of vaginal or utero-vaginal douches, whether by syringe or fountain irrigator, I have decidedly modified my opinions within the last year or two. Formerly I regularly insisted on their use twice or thrice daily, when any person, either nurse, or even experienced intelligent relative or neighbour, could be got to do the duty. I have used carbolic acid, Condyl's fluid, boracic acid, solution of mercuric perchloride, etc.; the first three I had used for over seven years. Now, I have often seen prolonged after-pains, uterine colic, and sometimes pelvic cellulitis from the use or abuse of these procedures. I latterly have relied on thoroughly bathing the external parts with the perchloride solution 1 to 1000, and in nine out of ten cases use no douche. Dr Thomas of New York has, I read, recently come to a like conviction regarding the non-necessity of the injection. We must give all credit to the physicians of the Maternity for the good results they have achieved from the plan now decried, but must, at the same time, remember it is for the general practitioner who, in 70 per cent. or more of his cases, has no skilled nurse to carry out his instructions, that our counsels are given. After the establishment of puerperal mischief, tenderness, increased pulse, headache, increased temperature, etc., etc., the treatment is wholly different; then at once, if need be, clean out the uterus by antiseptic hand, and wash not only the vagina but uterus also. Unless a nurse is thoroughly trustworthy this is the physician's duty. I have, long ere mercuric chloride was talked of, seen the best results from such a proceeding, using glacial carbolic acid, glycerine, and water, or solution of potas. permang., boracic acid, saturated solution reduced by one-half by hot water, or iodine ʒij. of tinct. to pint, or thymol solution. I have

also used the perchloride 1-1000 or 2000. Salicylic acid or chloral hydrate I have not used; I could understand the benefit of the latter in pelvic infiltration with great tenderness, if threatening septic absorption. But in addition to these a vaginal suppository of equal parts of iodoform and trisnitrate of bismuth, or a frequently changed pledget of wood wool or tow saturated with eucalyptus oil, is necessary. Salicylic wool, or iodoform wool, or mercuric gauze should be used as napkins. Bottini, of Pavia, has recently recommended sulpho-carbolate of zinc in 2 to 5 per cent. solution of alcohol and water, or distilled water, as a non-poisonous non-irritating fluid of high value for vaginal injection or irrigation. If it is really a reliable antiseptic, it should take precedence of mercury, as we know that, at least experimentally, a serum albuminate is likely to be formed even with a 1 in 500 solution, which is four times the usual strength employed. A few words in conclusion regarding the internal treatment by antiseptics. I can with confidence recommend carbolate of quinine. We all from experience place quinine high. The sulpho-carbolates, mostly of soda, I have seen of decided use; salicylate of soda is often beneficial. Tincture of iodine I have seldom employed; theoretically it should be good. Arsenic, as potas. arsenit., might be used advantageously. Turpentine, formerly of high repute, possibly owed part of its renown to its antiseptic qualities. But clinically we must resort to adjunctive treatment, reduction of temperature by diaphoresis; the wet pack, of great value in suitable cases; aconite, digitalis, veratrum, ergot, etc., I need but mention. Sometimes iron is very beneficial, possibly by its action on the red corpuscles it acts as an antiseptic of the best kind. It must be borne in mind that "certain micrococci will not grow in weak, but flourish in strong solutions of so-called germicides" (Klein). I cannot quite accept Dr Barbour's theory that septicæmia or septic infection may be cured by antiseptics. Indirectly, by decreasing the multiplication and activity of pathogenic micrococci, they permit the patient to live through the attack, they may hinder further development, but do not destroy existing micro-organisms. Healthy blood will probably be found the best germicide.

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING III.

Thursday, January 14, 1886.—Dr LOCKIE in the Chair.

Dr Lediard showed a boy, aged 7, on whom OSTEOTOMY had been performed for a rickety condition of both legs. Both the tibiæ had been chiselled through, the fibulæ broken. Marked improvement had taken place, although the limbs were not perfectly straight, owing to the femora being also involved.

Dr Lediard showed TWO TUMOURS of the PAROTID GLANDS of

youths, aged 19 and 20 respectively. Microscopically, both tumours showed cancer cells and also cartilage.

Dr Greenlees read a paper on OBSERVATIONS WITH THE SPHYGMOGRAPH ON THE INSANE. After a few introductory remarks on the circulation generally in insanity, he pointed out that in mania, especially in the more acute types of the disease, the line of ascent in the sphygmogram is generally perpendicular, and the percussion wave sharply pointed, while in the more chronic types the ascent line is more slanted and the percussion wave more rounded. In melancholia, on the other hand, there is evidence of weak cardiac action and imperfect filling of the vessels, as shown by the low ascent line and the prolonged line of descent. In general paralysis the condition of the circulation is full of interest, and by the tracings obtained, the author was able to recognise the three different stages into which the disease is usually divided. It is, however, only in the second stage that the characteristic "plateau" like summit (as described by Voisin) is obtained in the tracing. This prolongation of the percussion wave indicates a marked increase of arterial tension, and is similar to the condition of the vessels usually found in chronic Bright's disease. In epilepsy it was only in long-standing cases, or in the *Status Epilepticus* that he was able to obtain tracings generally described as indicating a bad condition of the arterial walls. The ascent line is low, the summit rounded, and the line of descent prolonged, generally presenting a distinct dicrotic wave, which sometimes ascends higher than the percussion wave. In dements and imbeciles the sphygmogram indicated a sluggish condition of the circulation, and these cases presented other evidences of impaired cardiac action, such as swollen and livid extremities. Cases of insanity due to syphilis have a tracing in many respects similar to that of general paralysis. The paper was illustrated with 80 original tracings obtained from patients in the Garlands Asylum.

Dr Barnes congratulated so young a Society on having a paper read showing original work. The sphygmograph was of use in educating the finger. In general paralysis the second sound of the heart was accentuated.

Dr Lockie related a case of epistaxis in which no albumen was discoverable in the urine, while the sphygmograph showed increased arterial tension. Subsequently albumen developed, and the patient died of uræmia. The pulse tracing in enteric fever resembles that of dementia. He had noticed increased tension in the pulse of persons labouring under temporary depression of spirits with sluggish action of the liver.

Dr G. F. Smith read a CASE OF COMPOUND DEPRESSED FRACTURE OF THE SKULL, IN WHICH THE BONE WAS ELEVATED; DEATH ON THE NINTH DAY AFTER INJURY. The injury was a little below the centre of the left parietal bone. Immediately opposite, there was a fine linear fracture of the right parietal bone, apparently caused by contrecoup.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on *Materia Medica*, Edinburgh School of Medicine, etc., etc.

THE ANTIPHLOGISTIC ACTION OF MENTHOL.—Dr Selwyn A. Russel of Albany, New York, writes to the *Medical Record*, Nov. 21st, 1885, to call attention to the use of menthol as an antiphlogistic and anodyne remedy in superficial inflammations, in which he says its effects are truly remarkable. My discovery of its value in such conditions was quite accidental; a patient who was suffering from acute aural perichondritis, failing to obtain relief from the usual anodyne applications, bethought himself of menthol, which he applied by means of the ordinary pencil; the relief from pain was immediate, and from that time, to my surprise, the inflammation quickly subsided. I have since made frequent use of it, and I believe inflammations have aborted which without it would have ended in abscess, and in addition the pain, itching, heat, and swelling have been considerably reduced. I have used the ethereal solution (alcohol is also a good solvent) in the strength from 10 to 50 per cent., applying it by means of a camel's hair pencil. The application may be made two or three times a day, or more or less frequently, according to the indications. A solution in one of the fixed oils may be used on mucous membranes. I have observed the good effects of menthol, more especially in boils, carbuncles, excessive inflammation following vaccination, etc. The employment of this substance in superficial neuralgias for its anodyne effect alone is often very satisfactory, but its use in this way has been noticed and written upon by others.—*Therapeutic Gazette*, January 1886.

THE PHYSIOLOGICAL ACTION OF GRINDELIA ROBUSTA.—Dr J. A. Buffington arrives at the following conclusions in a graduation thesis as to the physiological action of *Grindelia robusta* (*American Journ. Med. Sci.*, Jan. 1886). *Grindelia* was found to be not actively toxic. The lethal dose of the fluid extract, the preparation used in the experiments, was ʒij. to ʒiij. for rabbits weighing 28 oz. to 32 oz., or ℥x. to ℥xv. for frogs weighing one ounce, and the time required to cause their death was one to three hours in the former and half to one hour in the latter. *Grindelia* first lessens, then destroys sensibility, the action beginning in the peripheral end-organs, then involving the trunk of the nerves, and ultimately the spinal cord. Mobility is affected in the same order, the paralysis beginning in the nerve terminals, then involving the nerve trunks, and finally the motor centres in the spinal cord. The heart is slowed because of an increase of inhibition due to stimula-

tion of the cardiac inhibiting centre, and the blood pressure is raised chiefly by stimulation of the vasomotor centre in the medulla. The respiration is increased in frequency by an action of the agent on the respiratory centre, and also on the terminals of the pneumogastric in the lungs. Toward the end the respiration becomes slower and is jerky in character, and death ensues by arrest of respiration, the heart continuing its action sometime longer. The cerebral effects of grindelia are quite pronounced. A condition of narcosis, more or less profound, ensues as the effects of the drug develop. This action appears to be due to a direct impression made on the cells of the cerebral lobes. A marked increase of the urinary secretion was noted in all the animals experimented on. As some constituents of the drug appear in the urine, a direct action on the renal structure may be admitted. The increase in the blood pressure is another factor, and it may be the chief one.—*Therapeutic Gazette*, January 1886.

TUBERCULAR MENINGITIS CURED BY IODOFORM.—A Swedish physician, Dr Emil Nilsson, alleges that he has cured an undoubted case of tubercular meningitis by frictions on the shaved scalp with iodoform ointment (1 to 10). The patient was a boy, aged eight, whose mother had a family history of phthisis, and four of whose brothers and sisters had died from tubercular meningitis. The symptoms in this child's case were similar to theirs—headache, torpor, convulsions, strabismus, and pyrexia. He was at first treated with calomel and iodide of potassium, but did not improve, and after having been under treatment for a week became distinctly worse, being unable to take food or medicine. The pallor of the face which had pre-existed gave way to flushes of the cheeks. The child threw himself out of bed, and presented several clonic spasms of the limbs and of the facial muscles. The head was then shaved, and iodoform ointment rubbed in, an oilskin cap being put on. The friction was repeated three or four times in the day, and the next day there was a decrease in the convulsive movements, the sleep was calmer, and the spasmodic contractions which had previously been excited by the slightest noise now ceased to be so. Consciousness shortly afterwards returned, and the child's face became of a more natural colour. This, however, was accompanied with a severe coryza, redness of the lips, and an irritable cough, the breath smelling strongly of iodoform. The ointment was discontinued, and syrup of iodide of iron given. The unpleasant symptoms rapidly disappeared, and the child was soon running about in good health.—*Therapeutic Gazette*, January 1886.

ACTION OF MERCURY ON THE BLOOD IN SECONDARY SYPHILIS AND IN ANÆMIA.—The introduction of the hematimetric method into the experimental researches of therapeutics insures a precision and exactness of investigation unattainable by the older observers. The influence of the salts of mercury on the circulation—this old and highly important topic—has been recently the subject of a

most thorough and strictly scientific clinical study by Dr L. Gaillard (vide *Archives Générales de Médecine*, Nov. 1885). The subjects on whom the first series of observations was instituted comprised—(1.) Syphilitic patients without or with a slight anæmia. (2.) Syphilitic individual with an advanced state of anæmia. (3.) Syphilitic individual also with an advanced state of anæmia, but in a condition of mercurialization. Gaillard's conclusions are as follows:—In the first group a diminution of the number of blood-globules and of the hæmoglobin could be observed in the course and at the end of the mercurial treatment. In the second group a favourable result could rapidly be noted, both the number of blood-globules and the hæmoglobin increasing considerably and in a short time. The subject of the third observation was not improved. The anæmia had no tendency to disappear, even after the effects of mercurialization had passed away. There was an evident intolerance of the drug, which must be borne in mind, both regarding prognosis and the estimation of the therapeutic result. Comparing these results, we see that the second category, representing syphilis and anæmia, profits most from the tonic and reparative action of mercury. It is, hence, in those patients that we can look upon mercury as a restoring and augmenting agent of both blood-globules and hæmoglobin. This refers only to the red blood-corpuscles; the white ones were, nevertheless, counted by Gaillard in each instance. Their number appears to diminish under the influence of mercury, as has also been held as early as 1874 by Wilharchewite. [Still it appears hazardous to make any definite statement in this respect. We all know the instability and variance locally and numerically of the white blood-corpuscles. Remembering their abundance in persons affected with even slight suppurations, we would not be surprised to see them decreasing in a woman suffering with vaginitis after the cure of the local trouble.] The second series of observations refers to the action of mercury on the blood in anæmia. The patient took ordinary doses of either the protoiodide of mercury or the sublimate. The following results are comprised in the following theses:—(1.) The number of red blood-globules can decrease in anæmic persons under a mercurial treatment in the beginning without reattaining the original numerical status. More frequently, however, they increase in number progressively up to the fourteenth day, when a slight reduction takes place again. (2.) The hæmoglobin increased daily and progressively up to the twenty-fourth day; then it is reduced towards its original figure, but surpassing it still if the treatment lasts for several weeks. (3.) The hæmoglobin increases more considerably in proportion than the red blood-corpuscles. It increases even when the latter diminish in number, so that mercury can be placed alongside of those metals which elaborate hæmoglobin. (4.) The bodily weight increases nearly constantly under a mercurial treatment. Still we are unable to establish an exact relation between this phenomenon and the condition of the blood.—*Therapeutic Gazette*, January 1886.

ON THE PROPERTIES AND PREPARATION OF PEPTONE. — The medicine, or rather aliment, called peptone has of late been so frequently referred to in the medical press that it is well to gain a clear perception of its properties and method of preparation. We abstract from the *Pharm. Zeit.* of Nov. 25 the following instructive items on peptone:—It is usually assumed that peptones are gained by hydration from albumen, such as egg-albumen, caseine, proteine, and meat-albumen, and that this hydration takes place if pepsine, pancreatic juice, or papaya juice, in an acid solution, are made to react on the mentioned albuminous substances. This assumption is all the more justifiable, as pepsine can be retransformed into albumen by rehydrating substances. It was believed for a long time that the latter reaction took place within the human economy, until Hoffmeister proved experimentally that peptone passes the kidneys unchanged unless the white blood-corpuscles of the intestinal mucous membrane have appropriated it. According to this author, then, no such retrograde change as formerly believed takes place. The mode of preparation of peptone is very variable. Some use eggs, some meat, others again milk, while papaya juice, pepsine, or pancreatic juice are selected by still other manufacturers. It is clear that the preparations of peptone are consequently very different in concentration and physiological action. The chemical properties of an eligible peptone preparation are as follows:—(1.) Mixed with at least two parts of water, peptone ought to form a perfectly fluid non-gelatinous liquid. (2.) A 10 per cent. solution should not grow turbid on the addition of acetic acid, muriatic acid, or ferrocyanide of potassium. (3.) On the addition of a solution of picric acid or of tannic acid, yellow flocculi should appear. (4.) With sulphate of copper in strong potash solution, a violet colour should result.—*Therapeutic Gazette*, January 1886.

PERISCOPE OF GYNÆCOLOGY AND MIDWIFERY.

By J. MILNE CHAPMAN, Inverness.

THE REMOVAL OF TUMOURS OF THE ABDOMINAL WALL WITH THEIR PERITONEUM (Sänger, *Archiv f. Gynäk.*, xxiv. 1).—In this paper the writer deals with the removal of tumours of the belly wall so closely and extensively applied to the peritoneum that this membrane can only be preserved uninjured by a difficult dissection, after which a large thin sheet of peritoneum, free from its main vascular connexions, will be left. In such circumstances some operators have adopted the easier course of cutting away the tumour with its peritoneal covering, and taking great pains to bring together the edges of the peritoneal wound, leaving the skin which covered the tumour as a large loose bag over the stitched-up incision. Others have filled up the gap by stitching omentum into the wound, without great success. Sängér here publishes a

case in which he simply stitched together the margins of the incision through the skin and muscles, leaving the large surface from which the tumour had been removed uncovered with peritoneum, so that after closure of the wound a great part of the anterior abdominal wall was left bare of peritoneum. Three similar cases have been published before—one by Esmarch, two by Sklifossowsky. All four were successful. In only one of them (Esmarch's) was drainage employed. In order to determine the behaviour of the parts affected when this course had been adopted, Dr Säger has made experiments upon animals, and he finds that just as after a destruction of skin, healing takes place, and fresh epidermis is formed, so after removal of part of the peritoneum new endothelium is produced. The paper concludes with a general survey of all the cases of tumour of the abdominal wall known to the author.

HYDROCHLORATE OF COCAINE IN THE VOMITING OF PREGNANCY.—Weiss, of Prague, has used this remedy successfully in a case of vomiting of pregnancy which had resisted all previous attempts at relief. The patient was weak and anæmic, of a nervous disposition, and had suffered in three previous pregnancies from persistent vomiting: in the present pregnancy her condition was serious. Weiss prescribed:—

R. Hydrochlorate of cocaine, . . . gr. ij.
 Alcohol, enough to dissolve,
 Water, ... ℥v.

S.—One teaspoonful every half-hour.

After the sixth dose three tablespoonfuls of milk were well borne; after the eighth, a cup of broth with egg, without vomiting. After the sixteenth dose the patient ate with relish chicken broth, slices of white chicken meat, and drank a glass of wine, without vomiting. The drug was then withdrawn for a time, owing to an increased frequency of pulse and respiration; but hourly doses were subsequently given, with the result of entirely checking the vomiting and enabling the patient to regain her former strength.

PURULENT ENDOMETRITIS DURING PREGNANCY (Douat, *Archiv f. Gynäk.*, xxix. 3).—This disease subsequent to confinement is an ordinary occurrence, but till the present time no case has been recorded of its existence during pregnancy. A woman, aged 27, in whom menstruation had occurred since the age of 14, and whose history showed no syphilitic infection or other constitutional disease, entered the clinic at Leipsic, where pregnancy at term was diagnosed. Labour was rapid and easy. The amniotic fluid was of a dark green and yellow colour, and contained numerous pus corpuscles. Examination of the placenta and its membranes showed the presence of adhesions and an unnatural condition of the amnion and chorion, between which pus was found. Upon

the borders of the placenta the true decidua was infiltrated with traces of thickened pus, and the decidua reflexa was likewise infiltrated with pus. There was no trace of the gonococcus, and it was evident that the purulent process had begun both in the uterine and ovular decidua. The purulent cells were found scattered between the membranes, and had given rise to small abscesses. As to the cause of the condition, it may in part be attributed to blenorrhagic infection and partly to attempts to produce abortion. The patient for a long time had been affected with leucorrhœa, and presented other physical signs of endometritis. The chief interest in the case is, however, the fact that purulent endometritis could exist during pregnancy without interrupting its course.

- THE NATURE OF THE SECRETIONS OF THE FEMALE GENITAL ORGANS (Ménière, *Journal des Coun. Méd. Rev. de Thérap.*, 1st August 1885).—The following are the writer's conclusions:—1. The secretions of the human organism are, as a rule, alkaline. 2. Those of woman are less alkaline than those of man. 3. The vulvo-vaginal mucus tends to become more acid as the constitution is weak, or when it is produced as a result of morbid processes. The uterine mucous secretion under the same condition becomes more alkaline. The state of health, therefore, corresponds with a slight acidity of the vulvo-vaginal secretions (with the exception of the glands of Bartholini), and with a moderate alkalinity of the uterine mucus. 4. The changes of composition are proved by clinical facts, chemical analysis, microscopical examination, and particularly by the cure of sterility from certain thermal causes. 5. To changes of composition correspond hypersecretion, a composition immediately different, and the appearance of micro-organisms; moreover, the uterine mucous membrane constitutes a region in which parasites, neither animal nor vegetable, can ever be found. 6. Notwithstanding the variations in composition, the mucus secreted by the muciparous follicles and the vaginal mucous membrane always maintains an acid reaction. 7. The mucus secreted by the follicles of the cervical and corporeal cavities, and finally that of the vulvo-vaginal glands, remain invariably alkaline.
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WHEN TO TIE THE UMBILICAL CORD (Engel, *Centbl. f. Gynäk.* No. 46, 1885).—The writer strongly recommends that the cord should not be tied till all pulsation in it has ceased, and in the course of his paper mentions the following facts as evidence of the importance to the child of the small quantity of blood thus saved to it:—"He contrasts the mortality of all the premature children born in the Klausenburg Hospital during the last eight years, during the first four of which it was the custom to ligature the cord immediately on the birth of the child, while during the latter four the plan of treatment he advises was practised. During the first period there were 90 premature undersized and underweighted children born, of whom 17, or 18·88

per cent., died within ten days of birth, while during the latter period there were 74 such births, of whom 10, or 13·51 per cent., died within the same time. In cases where the mother became feverish, a wet nurse was had recourse to. "This striking difference of mortality can only—in the absence of any other visible cause—be attributed to late ligature of the cord."

LACTATION AND MEDICAMENTS (Fehling, *Bull. de Therap.*, 30th August 1885).—If two grammes (half a dram) of salicylate of soda are administered, this substance is readily found in the urine of the new-born. The passage is specially marked when the drug has been absorbed two hours before the nursing. Iodide of potassium acts like the salicylate of soda. Iodoform, even when used in very small quantity, passes into the milk. A simple sprinkling of this drug upon the vulva is sufficient to secure its appearance in the mammary secretion. It was not so with corrosive sublimate, of which it was possible to discover in the milk only very small quantities—so small that it was impossible to estimate them. The narcotics are without effect upon the nursing. The strongest doses of opium or of chloral administered to the nurses have not produced any special physiological effect upon the nursing. Atropine tested upon animals produced dilatation of the pupil in the nursing only when the maximum therapeutic dose was exceeded.

EXCESSIVE VOMITING OF PREGNANCY INSTANTLY RELIEVED BY ETHER IRRIGATIONS UPON THE EPIGASTRIUM (Mendel, *Archiv. de Tocol.*, September 1885).—A young woman, primipara, of feeble constitution, had frequent vomiting since the second month of pregnancy. At the fifth month the vomiting became more persistent, and was accompanied in the intervals with nausea, fainting, and general malaise. In a few hours they became so frequent that they succeeded without interruption, producing syncope, absolute prostration of power, noises in the ears, chills, cold and profuse sweats, frequent and filiform pulse. Her life was manifestly in danger. Means the most varied to arrest this vomiting had been employed without result. In their turn anti-spasmodics had been used (ether, valerian, musk), then opiates, chloral, carbonated and iced drinks, iodine, internally and externally, blisters upon the epigastrium, hypodermic injection of morphine, ether, etc.; ultimately irrigation of ether upon the epigastrium was tried. The effect was instantaneous. A single irrigation sufficed to cut short the vomiting. The patient drew a few long breaths, said she was cured, and felt perfectly well. Later the vomiting returned twice, and each time the ether irrigations arrested all trouble.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. ALLAN JAMIESON, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

PSORIASIS.—Greenough remarks that quite a fair proportion of cases of psoriasis are mistaken for, and treated as, syphilis. He mentions the case of a respectable middle-aged woman, who showed a typical psoriasis, with a history of its having first appeared before the age of puberty, and of having been affected with it off and on ever since, who said that she would follow out any treatment he ordered, but she hoped he would not give her mercury, as she had been salivated six times without any improvement in the disease. One quite characteristic symptom of psoriasis of the scalp is the existence of a band of hyperæmia, from three-eighths to half-an-inch in breadth, running along the forehead contiguous to the margin of the growth of the hair. This band of hyperæmia is typical of psoriasis, and is to be considered in questions of differential diagnosis between psoriasis and eczema capitis. It is never seen in the latter. On the general integument a squamous syphilide is one of the eruptions most likely to show a resemblance to psoriasis. There is a decided difference in the first appearance of the two. Psoriasis invariably begins as a very minute point of simple hyperæmia, and this lasts several days as such. In the macular syphilide it is otherwise. In twenty-four, or even twelve hours from the time when the cutis was seen to be normal, a crop of macules from the size of a small shot to that of the nail of the little finger will appear. Again, when the syphilide fades, it leaves for a considerable time a decided pigmentary stain, but no pigmentation remains after the disappearance of psoriasis. When chrysarobin is used in the treatment of psoriasis its application must be persisted in until the part affected is stained as deeply as the sound skin surrounding it. Till this occurs the morbid process has not been overcome.—*Boston Medical and Surgical Journal*, 10th September 1885.

HERPES ZOSTER AND PARALYSIS OF MOTOR NERVES.—Strübing has directed attention to this occasional concomitant of zoster. In the majority of cases the eruption of zoster precedes the paralysis. Only rarely does the zoster develop as a sequence of the paralysis. The causal connexion between the two is evident in the one series; in the other it is less easily understood. The circumstances under which zoster can be accompanied by paralysis of motor nerves are plainly various. Leaving those cases out of consideration in which zoster is a result of the inflammation of the intervertebral ganglia, it may likewise arise from disease both of the central and peripheral nerve systems. In central lesions the disease may extend from trophic to motor tracts; or, conversely, the motor may be primarily, the trophic apparatus secondarily affected. Numerous

cases are cited, but special attention is directed to those in which zoster of the fifth nerve was associated with facial palsy. The author concludes as follows:—"The connexion between zoster facialis and paralysis of the facial nerve must in many cases be thus explained, that an inflammatory process constantly advances from one nerve to the other by means of anastomosing filaments, and that thus the peripheral affection of the sensitive nerve which induces zoster has superadded a peripheral facial paralysis. And inversely, also, in other instances the connexion between the two lesions may possibly be different. One of the most prominent causes of peripheral facial paralysis is a so-called chill; and the observations of Paget, Johnson, and others have proved that facial zoster may be incurred by a similar cause. The same injurious influence, therefore, which affects the trigeminus, and leads to the development of zoster, induces, through its action on the peripheral branches of the facial nerve, paralysis of the muscles it supplies."—*Deutsches Archiv für Klinische Medizin*, October 1885.

LACTIC ACID AS A CAUSTIC.—Mosetig-Moorhof recommends this in lupus fungating growths, rodent ulcer, and papillomata. Its value consists in its sparing the sound tissues. The simplest mode of application consists in mixing equal parts of lactic acid and finely powdered silica. This is spread thickly on gum paper, and retained in position for twelve hours, removed, and renewed twenty-four hours afterwards. From five to seven applications serve to cure lupus.—*Centralblatt für Chirurgie*, 12th November 1885.

LANOLIN, A NEW BASIS FOR OINTMENTS.—Oscar Liebreich has investigated a series of fats which bear the same relation to cholesterin as the more commonly accepted fats do to glycerin. Hartmann and Schulze were the first to notice that in the fat of sheep's wool the fatty acids were united to cholesterin. How extensively this new fat was distributed throughout the animal kingdom there was no available means of ascertaining, till Lieberman discovered a characteristic reaction for cholestol, a body closely related to cholesterin. The reaction is a simple one. The fat to be investigated, and a very small quantity is needed, is dissolved in acetic acid anhydrid. On the addition to this of concentrated sulphuric acid there is produced first a rose tint, which soon changes into a deep blue and then green tint. Liebreich's supposition that this cholestol reaction would also be useful for the detection of cholesterin fats proved correct on being tested by him. The keratin containing tissues were found to exhibit this cholestol reaction. The subcutaneous fatty tissue, on the contrary, either did not show it, or merely in a slight degree; the fat of the liver and kidneys very markedly. The next point to be ascertained was whether the cholesterin fats belonged to the tissues as such, or were produced by glandular secretion. He found that the feathers of the fan-tailed pigeon, in which there are no oil glands attached to the feather glands, contained this cholesterin fatty substance, and in-

fers that it is secreted with the feathers. In the hairs of the sloth, which has no sebaceous glands in connexion with the hairs, the cholesterin fat was detected. This fat therefore bears a distinct relation to the keratin containing tissues, and its interest in a therapeutic point is connected with this. In the salves and plasters in common use the neutral glycerin fats have been used, and of late mineral fatty substances, as vaseline, have come into use. The fat salves spontaneously, and especially when applied to the skin, tend to decompose, and thus cause irritation. Apart from this, even with pure fat, the absorption of medicinal substances is imperfect. Vaseline does not become rancid, but it prevents the introduction of medicinal substances through the skin. The cholesterin fat contained in sheep's wool is easily applied to the skin, and from its origin in tissues containing keratin, Liebreich believes that it would be a means of obtaining absorption through the skin. Water takes up 100 per cent. of it, and to this combination he has given the name of lanolin. This is a clear, yellowish plastic salve. It does not tend to decompose. It absorbs glycerin easily, and the mixture can then be readily combined with any other fat. Lanolin has a slight odour. When a 5 per cent. carbolic lanolin salve is rubbed on the skin, a piece the size of a pea being employed, numbness without any caustic action is produced in from one to two minutes. When a lanolin sublimate salve, 1 in 1000, is rubbed into the skin, in a few minutes a characteristic metallic taste is experienced.—*Berliner Klinische Wochenschrift*, No. 47, 1885.

LYMPODERMIA PERNICIOSA.—This is the name given by Kaposi to a new disease described and figured in the *Med. Jahrbüchern der K. K. Gesellschaft der Aerzte in Wien* 1885. The patient was a woman aged 39. When she came into hospital she had a universal chronic eczema. There was a remarkable pachydermatous condition of the skin of the face and neck, with exemption of the lower extremities. In course of time a diffuse infiltration of the corium appeared, with evidences of increasing general leukemia, with enlargement of the spleen, etc. The skin over these indurations became reddened, and losses of substance took place, the size of a crown piece, from exfoliation. Death occurred suddenly. The autopsy showed lymph tumours in nearly all the organs, and in the skin.—*Monatshefte für praktische Dermatologie*, September 1885.

PRURITUS VULVÆ.—Martineau notes that this arises sometimes in the course of affections unconnected with the vulva, at others during the evolution of a disorder or lesion of this part. In the first class are intestinal worms, the oxyuris in particular; these wander at night over the neighbourhood of the anus and genital organs. They should always be looked for there and then, especially in children, when there is an absence of any direct cause. Tinea tonsurans and the pediculus pubis are other causes. Affections of the bladder, vegetations and polypi of the urethra may lead to it.

Glycosuria, also, either temporary in wet nurses, as in those who take much sugar, or permanent, as in diabetes. In the second category may be ranged pruritus, consecutive to various primary or secondary inflammations of the vulva, which may be simply local, or proceed from a general diathetic or constitutional cause, as tuberculosis, eczema, herpes, psoriasis, lichen, epithelioma. Vulvar pruritus may be purely nervous, and then appears without any manifest lesion of the mucous membrane or skin. At times may be associated with urticaria. The diabetic form is best treated with the effervescent citrate of lithia, with the addition of a little arseniate of soda. Locally, during the acute stage, lotions of bromide of potass or of chloral are recommended, and in the chronic phases a weak solution of corrosive sublimate and alcohol.—*Annales Medico-Chirurgicales*, Sept. 1885.

SURGICAL PERISCOPE.

By A. G. MILLER, F.R.C.S.E.

FÆTID ABSCESSES OF THE LIMBS.—M. Nepveu, of Paris, records two cases of putrid abscess of the arm, without any apparent source of infection. He considered that the bacteria, which were found in great numbers, had not found their way to the abscesses through the circulation, but had remained dormant after a previous injury. Both patients had been wounded in the places where the abscesses formed in the war of 1870-71, and the abscesses appeared in 1884. He considers these cases establish that bacteria may remain dormant for many years. The abscesses were connected with necrosed bone.—*Revue de Chirurgie*.

PATHOLOGY OF SUPPURATION.—M. Socin, of Bale, considers suppuration to be produced by the presence of some microbe. In 76 cases of unopened abscesses M. Socin found the two varieties of staphylococcus (white and yellow) in 68, in 6 he found the streptococcus also, and in the remaining 2, the last alone. His assistant, M. Garri, experimented upon himself with the pus from osteomyelitis, and got only abscesses as the result of injections from cultivations. M. Garri had seventeen cicatrices to show as the result of his experiments from two inoculations. M. Socin considers that the germs which cause osteomyelitis, whitlow, boils, etc., are identical.—*Revue de Chirurgie*.

NERVE SUTURE.—M. Nicaise, of Paris, reports a most interesting and remarkable case of a young woman who had a wound of the forearm which implicated the median nerve. M. Nicaise cut down on the severed ends and united them, after paring the ends. This was six weeks after the wound. The day after the operation slight return of sensation was perceived. After four months there was perfect recovery. M. Nicaise considers the immediate return of sensibility (nerve conduction) as a contradiction to the teaching of

physiology. He suggests that the irritation produced by the presence of the sutures causes greater activity in the anastomotic filaments, till the nerve fibres are renewed at the sutured portion. His conclusion is, therefore, that nerve suture is a good operation. It hastens the return of sensibility, and is quite safe if performed with antiseptic precautions.—*Revue de Chirurgie*.

COMPARATIVE VALUE OF ILIAC AND LUMBAR OPENINGS IN CANCER OF THE RECTUM.—M. Reclus, of Paris, gives the following five points:—1. Artificial anus behind is less disgusting, and can be borne with less inconvenience, and can be more easily kept clean. 2. Contraction of the orifice, with inversion, is more common in the iliac anus, though occurring in the lumbar also. 3. In cases of lumbar colotomy there is apt to be an accumulation of feculent matter between the opening and the stricture. This is apt to cause irritation and peritonitis. To prevent this, occlusion of the distal portion of the bowel by suture is recommended. 4. As the descending colon is fixed, and the sigmoid flexure is movable, and its position uncertain, the former is easier found. M. Reclus considers that the S. flexure is not so movable as is supposed. 5. In lumbar colotomy the peritoneum is not opened. For all that M. Reclus says that the risk of the iliac operation is less, and therefore he prefers it.—*Revue de Chirurgie*.

ILIAC ANUS WITH OBLITERATION OF DISTAL PORTION OF BOWEL.—M. Pollosson, of Lyons, recommends securing to the skin the proximal, and completely shutting up the distal portion of the bowel (S. flexion) after it is opened. This procedure secures greater certainty of rest to the cancerous rectum. M. Pollosson reports one case in which the disease seemed to improve after the operation. (Such an operation opens up a possibility of cure in certain cases where extirpation of the rectum might be attempted after the iliac anus has been established. It is evident that in any case great benefit would likely follow the sigmoid operation. If it is done in two stages, the first antiseptic, there should be very little risk.)

OSTEOTOMY OR OSTEOCLASIS FOR GENU VALGUM.—M. Demous, of Bordeaux, advocates the latter operation (with M. Robin's instrument), because the fracture is sub-periosteal and there is no risk to life, there being only a simple fracture to deal with.—*Revue de Chirurgie*.

RUPTURED BOWEL WITHOUT EXTERNAL INJURY.—M. Chavasse, of Paris, gives the following indications:—1. Continuance of shock; 2. Acute and persistent pain; 3. The manner and force of the blow may give considerable assistance. Treatment should be abdominal section, with suture of wound, formation of artificial anus, or resection of a portion of bowel. After peritonitis has set in it is too late to interfere.—*Revue de Chirurgie*.

Part Fifth.

MEDICAL NEWS.

ROYAL COLLEGES OF PHYSICIANS AND SURGEONS, EDINBURGH.—The following gentlemen passed their final examination for the double qualification in Medicine and Surgery at the sittings held in January and February 1886, and were admitted L.R.C.P. & S.Ed.:—William Booth, Edinburgh; David Davis, St George's, Littlemore, Oxon; Francis Philip Druman, Edinburgh; Robert Hall, Co. Down; William Singleton Fulshaw, Leicestershire; Abraham Haynes, Cork; Robert E. Kane, Cork; Frank Reginald Longden, Edinburgh; Michael J. Lee, Clifden, Co. Galway; William George Meade, Co. Cork; Herbert Peck, Southport; Nesbitt Robert Archer, Co. Down; Arthur E. Nevins, Liverpool; Hubert McKeon, Portobello; Arthur Wellesley Waler, Belfast; George James Elliot Trotter, Darlington.

ROYAL COLLEGE OF SURGEONS, EDINBURGH.—The following gentlemen passed their final examination for the degree in Surgery at the sittings held on 18th January 1886, and were admitted L.R.C.S. Ed.:—Francis Eldon Horsford Daunt, Dublin; Charles Strickland, South Kensington; Archibald Louis Fullarton Robertson, Edinburgh.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentleman passed his final examination for the degree in Medicine at the sittings held on 8th February 1886, and was duly admitted L. R. C. P. Ed.:—Alfred Fisher, Liverpool.

LIST OF SURGEONS on Probation in the Medical Department of the British Army who were successful at both the London and Netley Examinations. The final positions of these gentlemen are determined by the marks gained in London added to those gained at Netley, and the combined numbers are accordingly shown in the list which follows:—

Fifty-first Session—1st February 1886.

No.	Name.	Combined Marks.	No.	Name.	Combined Marks.
*1.	Yarr, M. T., . . .	5911	21.	Kendall, H. W. M., . . .	4408
2.	Mumby, L. P., . . .	5570	22.	Elkington, H. P. G., . . .	4364
3.	Melville, C. H., . . .	5494	23.	Gordon-Dill, R. C., . . .	4324
4.	Mills, B. L., . . .	5356	24.	Buchanan, J. B., . . .	4314
5.	Rayner, H., . . .	5184	25.	Skerrett, F. T., . . .	4307
6.	Genge, R. E., . . .	5018	26.	Adamson, H. M., . . .	4301
7.	Cardew, G. S., . . .	5006	27.	Ramsay, H. M., . . .	4299
8.	Renny, C. A., . . .	4935	28.	Lavie, T. G., . . .	4299
9.	Thiele, H., . . .	4911	29.	Rose, J., . . .	4271
10.	Cocks, H., . . .	4905	30.	Cox, R. H., . . .	4226
11.	Wilson, J. B., . . .	4877	31.	Brown, H. H., . . .	4191
12.	Lee, W. J., . . .	4849	32.	Corkery, T. H., . . .	4150
13.	Black, J. G., . . .	4818	33.	Locker, E. H., . . .	4142
14.	Kearney, J., . . .	4790	34.	Crooke, W. R., . . .	4018
15.	Henneasy, F. W., . . .	4773	35.	Squire, W. P., . . .	3930
16.	Saw, F. A., . . .	4659	36.	Walsh, C. L., . . .	3923
17.	Stokes, W. B., . . .	4489	37.	O'Donnell, J. J., . . .	3922
18.	Hall, F. W. G., . . .	4481	38.	Hayman, S. J. W., . . .	3893
19.	Kennedy, A., . . .	4413	39.	Hayes, J. P. S., . . .	3730
20.	Tate, G. S., . . .	4410			

* Gained the Herbert Prize of £20, with the Montefiore Medal and Prize of 20 guineas, the Martin Memorial Gold Medal, and the Parkes Memorial Bronze Medal.

LIST OF CANDIDATES who were successful for Appointments as Surgeons in the Medical Staff of Her Majesty's Army, at the Competitive Examination in London, on the 8th and following days of February 1886:—

No.	Name.	Marks.	No.	Name.	Marks.
1.	Fayrer, J., . . .	3045	31.	Garner, C., . . .	2325
2.	Davidson, J. S., . . .	3030	32.	Marks, G. F. H., . . .	2310
3.	Will, J., . . .	3025	33.	Scott, G., . . .	2310
4.	Fallon, J., . . .	2980	34.	Greig, D. M., . . .	2300
5.	Moir, J., . . .	2965	35.	Donaldson, J., . . .	2295
6.	Salvage, J. V., . . .	2840	36.	Holyoake, R., . . .	2275
7.	Bostock, R. A., . . .	2835	37.	Edege, J. S., . . .	2270
8.	Aldridge, A. R., . . .	2830	38.	Russell, J. J., . . .	2240
9.	Walker, C. P., . . .	2770	39.	Bate, L. A. F., . . .	2230
10.	Fowler, J. F. S., . . .	2735	40.	Allport, C. W., . . .	2215
11.	Macdonald, C. J., . . .	2695	41.	Barefoot, G. H., . . .	2205
12.	Burrows, J. R., . . .	2650	42.	O'Callaghan, D. M., . . .	2180
13.	Fatham, C. J. W., . . .	2650	43.	Windle, R. J., . . .	2175
14.	Austin, H. W., . . .	2630	44.	Wright, R. W., . . .	2150
15.	Clarkson, T. H. F., . . .	2610	45.	Knaggs, H. T., . . .	2140
16.	Kelly, R. E., . . .	2600	46.	Newland, T. R., . . .	2125
17.	Hore, H. St G. S., . . .	2580	47.	Foott, R. E., . . .	2110
18.	Donegan, J. F., . . .	2550	48.	Travers-Smith, V. E., . . .	2100
19.	Keatly, J., . . .	2550	49.	Mathias, H. B., . . .	2095
20.	Saunders, D. M., . . .	2550	50.	Hosie, A., . . .	2080
21.	Cottell, R. J. C., . . .	2530	51.	Buist, R. U., . . .	2050
22.	Bent, G., . . .	2480	52.	Watson, J. J. C., . . .	2050
23.	Cummins, H. A., . . .	2450	53.	Whaite, T. D., . . .	2050
24.	Deacon, J. G., . . .	2450	54.	Smyth, N., . . .	2040
25.	Marder, E. S., . . .	2400	55.	Trotter, W. J., . . .	2030
26.	Hennessey, D., . . .	2395	56.	Dowman, W., . . .	2020
27.	Whilty, M. J., . . .	2390	57.	Kiddle, W., . . .	2020
28.	Eckersley, E., . . .	2380	58.	Browning, T., . . .	2000
29.	Bailey, W. F., . . .	2360	59.	Cronin, C. S., . . .	1980
30.	Hall, R. J. D., . . .	2330	60.	Le Quesne, F. S., . . .	1940

HODGE & CO.'S NEW TRUSS.—A specimen of this truss has been sent us. It is very ingeniously constructed—rather too much so. We would certainly not like to be obliged to wear it. It is well calculated to go out of order, and in our opinion is much inferior for ordinary use to the common trusses at present employed.

SERVICE'S MILK SALT LOZENGES.—These lozenges have been analyzed by Professor Ferguson of Glasgow University, and he says they contain 1·58 per cent. of ash—an ash which “imitates the ash of milk as closely as practicable.” They are recommended as a food, but it is not easy to see what advantage they have over milk, and certainly the general public will never consent to substitute them for milk as an article of food. They are, however, very pleasant to take, and may be substituted for ordinary confections.

DIAGRAMS OF THE MEDICAL CORPS WITH AN ENGLISH ARMY IN THE FIELD.—Volunteer surgeons desirous of obtaining diagrams

showing the medical system of an English Army, with brief descriptive letters, can be supplied by Mr Lead, printer, Leicester, at 3s. 6d. per 100 copies. These diagrams are useful for instructing ambulance classes of volunteers.

A NEW METHOD OF LOCALIZING THE POSITION OF THE PARTS ON THE SURFACE OF THE BRAIN IN THE LIVING SUBJECT.—As is well known, various methods have been suggested for localizing the position of the convolutions and other superficial parts of the brain in the living subject. There are certain grave objections to nearly all of these, the chief being, firstly, that they are mostly founded on the position of the sutures, which are not always easy to find through the scalp; secondly, that although the grosser convolutions and fissures may be mapped out with tolerable accuracy, yet finer details, which are often exactly what are wanted in operative procedures, are left very much to chance; and, lastly, that the hair and the position of the patient in bed are serious incumbrances in making the various measurements. In cases of injury there is another objection, namely, that one cannot always recall on the spur of the moment the necessary rules laid down by the authors of these various schemes for finding the fissure of Rolando and other landmarks. What would be much more serviceable than such methods would be a means of automatically mapping out the position of internal parts by subdividing the scalp into a number of equal-sized areas. Such a method has lately been devised by Professor Hamilton of Aberdeen, which promises to yield very satisfactory results, and to render the localization of the surface of the brain, through its various coverings, a matter of greater certainty.

The method briefly consists in this:—A wire framework is made to cover the scalp so as to map its surface out into a number of squares. The special advantage of employing wire is that it can be passed through the hair and brought to lie in actual contact with the surface. One strong wire or steel band runs round the head from the root of the nose to the occipital protuberance. Another passes from front to back in the middle line, and between these run transverse and horizontal wires, which can be moved respectively backwards and forwards, or upwards and downwards, so as to adjust themselves to the size of various heads. These are so placed as to map out the scalp into a series of squares of as nearly as possible equal size. In order to find what each square corresponds to in the parts beneath, Professor Hamilton fixes the apparatus on the dead subject, and after it has been accurately adjusted, takes it off until the scalp and calvaria have been removed. It is now readjusted over the exposed brain, and the relationship of the framework to the underlying parts is recorded by means of photography. As each square has a definite number, the comparison of a large series of photographs gives most instructive results.

Another method of employing a similar apparatus is to localize the fissure of Rolando by means of one of the many methods recommended. From the horizontal band which runs round the head from the root of the nose to the occiput a wire passes upwards, and this is placed over the situation of the fissure. The parts anterior and posterior to this can now be subdivided into squares in the manner before mentioned, and the position of each recorded by photography. The numerous details so important in operations necessitating trephining, as well as the equally important matter of diagnosing the exact position of a cortical lesion, can by these methods be noted down in a chart, reference to which would alone be necessary in order to find where the underlying parts are located. The position, for instance, of the middle meningeal artery can be found without any difficulty.

We believe that before long Professor Hamilton will describe the method in detail, and give the result of his observations.

THE HABITUAL DRUNKARDS ACT, 1879, INEFFICIENT AND NOT ADEQUATE TO ACCOMPLISH THE IMPORTANT OBJECTS DESIRABLE, by A. Peddie.—We have received a copy of the *Proceedings of the Society for the Study and Cure of Inebriety* for Feb. 1886, containing a paper on the above subject by Dr A. Peddie, our much esteemed fellow-citizen. The paper is a valuable one, though perhaps not very encouraging to philanthropists, containing, among other important information, an account of nearly thirty years' experience of a struggle to obtain recognition of a medical fact, and justice for a class of persons who sorely require to be protected against themselves and those who trade on their diseased propensities. We would strongly recommend this paper to lawyers and members of Parliament. We think that Dr Peddie has no difficulty in proving that the present Act does little or nothing towards enabling friends and medical attendants to provide for the care and cure of diseased and also vicious drinkers.

DIRECT INTERSTITIAL MEDICATION BY DIELECTROLYSIS.—M. A. Brondel, of Algiers, brought forward a novel plan of medication at a recent meeting of the Paris Académie de médecine (*Rev. méd.*). By the term dielectrolysis (*diélectrolyse*) he refers to a process for making a nascent chemical substance pass through the tissues. For example, taking iodine, a body which is readily "dielectrolyzable," he applies to any desired part of the person a compress wet with a solution of iodide of potassium, and over it he places the negative electrode of a galvanic battery, the positive electrode being held against any indifferent part of the body. The iodine leaves the potassium, and, actually traversing the intervening tissues, rapidly arrives at the positive electrode, as may be ascertained by testing with starch-paper. In effect, therefore, this is a hypodermic, or rather interstitial (*intra-organique*) method, without wounding the skin or producing pain. As a great number of simple bodies

may thus be made to penetrate from one point to another, the practical applications of the new method may be very numerous and very important. By it the author has cured fibrous tumours of the uterus, a case of perimetritis, a rheumatic ovarian neuralgia, and several cases of chronic rheumatism. He has in view further trials upon parasitic and malignant tumours, diseases of the skin, syphilis, neuralgias, etc., and especially pulmonary consumption, on which latter he proposes to try the action of various mineral antiseptics, such as arsenic, mercury, fluorine, etc.—*New York Med. Journal*, 31st October 1885.

OBITUARY.

THE LATE DR ANGUS MACDONALD.

THE readers of this Journal must all feel most deeply the loss they, in common with the profession, the public, and especially the Edinburgh School of Medicine, have sustained by the premature death of this laborious and talented physician.

A detailed notice of his life, by one of his oldest friends, will appear in our next issue, in which also we hope to have the pleasure of publishing Dr Croom's eloquent and feeling tribute to his memory, and appreciative estimate of his special work as a gynaecologist and teacher of obstetrics, delivered on 24th February at the Obstetrical Society.

GEORGE GILLIES, L.R.C.S.

THIS worthy man had attained the age of 90, took the license of the Edinburgh College of Surgeons in 1816, and, with the exception of a visit to Canada and a short sojourn as a practitioner in Lowick, spent all his professional life in Coldstream in a quiet, uneventful practice. Wanting in energy and push, but full of kindness and cheerfulness, he passed a long life, chequered by sorrows and numerous bereavements, in patience, and died of natural decay, with unimpaired eyesight and marvellous freedom from any disease.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

DOLLAR, N.B., 16th February 1886.

SIR,—Recently looking over the discussion that took place a few months ago at the meeting of the Medico-Chirurgical Society, regarding the detection and removal of foreign bodies that had fallen into the trachea or oesophagus, I was much struck by the

similarity of result of exploration in the case adduced by Dr Shand, and one which I witnessed many years ago. A set of false teeth lodged in the œsophagus, causing evident uneasiness, and yet "notwithstanding the careful and skilful examination of each and all the staff of surgeons of the institution with probang, probe, and forceps, nothing was detected." "Prodigious!" a Dominie Sampson would exclaim, and yet what I am about to relate seems even more so.

In the winter session of 1834 or 1835 a young woman was admitted into the late Professor Syme's ward, and deponed as follows:—She was a domestic servant, æt. 20, and when struggling with her "neighbour" nine days ago for the possession of a padlock, she had put it into her mouth, and by an accidental gulp had swallowed it. The size of the padlock was described as about an inch and a half long, and one inch across—such as were usually attached at that time to carpet-bags. For a couple of days she underwent the very careful examination of the professor and his house-surgeon, but with a negative result; so was dismissed. The practical instruction that the professor then gave us—that it was impossible for any foreign body of the size represented to be lodged in the œsophagus or pharynx all that time, and have caused no discomfort, if not impediment in swallowing, the total failure of the repeated attempts made to discover its position, and all these circumstances taken together, plainly showed it must be an instance of those singular delusions to which young women of a hysterical temperament are liable, and against which we should be on our guard as to credence.

Dismissed, though not cured, and dissatisfied to boot, the young woman presented herself, along with many others, in Lizars's admission-room. Her tale was credited; by this time the lodgement had existed some two or three days more, but no discomfort. There was a considerable rivalry between Syme and Lizars; so the latter was by no means loth to accept the case, and maintained it was no hysteric delusion, as on examination he was quite satisfied that he had felt it or heard a "click." Bad luck, however, attended all his efforts at extraction, though forceps had been specially fabricated for the occasion. Strong in faith, however, a day was fixed for the *dernier ressort*, œsophagotomy. There was a large attendance in the theatre on that day, but, unfortunately, the operation did not take place. Catamenia or some other obstacle had intervened, so it was adjourned to Sunday. A full house. Another disappointment. Instead of the patient in comes "Johnny" in his happiest mood, *Dicete io Pæan*, holding aloft a pair of forceps which embraced the much-doubted, but *now* veritable padlock, fully as large as had been stated. The explanation given was this. Early in the morning the patient had had a severe fit of vomiting. The house-surgeon (I think he was a son of the former Professor Home), rightly conjecturing this might have

caused some displacement, introduced forceps with this successful result.

For the moral of the tale I will only quote the words of Duncan Gray, "Let doctors tell."

G. A. WALKER, M.D.

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F. J. B. Quinlan, M.D., L.R.C.S.I., F.K.Q.C.P., M.R.I.A.,
etc., etc., *Professor of Materia Medica and Therapeutics, Catholic University Medical College; and Examiner in Materia Medica and Therapeutics, Royal University, writes in the "Medical Press," July 15th, 1885.*

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Part First.

ORIGINAL COMMUNICATIONS.

I.—OBSERVATIONS ON THE SURGERY OF THE VASCULAR SYSTEM: ANEURISM, VARIX, LOSS OF BLOOD.

By JOHN DUNCAN, M.A., LL.D., F.R.C.S. Ed., Surgeon to the Royal Infirmary, Edinburgh.

I NEED scarcely say that my aim in these papers has not been to make an exhaustive study of the surgery of the vascular system, but rather to record matters of interest which may have been brought under my notice in a perhaps somewhat exceptional experience. In pursuance of this plan I purpose making a few remarks on those aspects of aneurism, varix, and loss of blood with which I have been more particularly concerned.

Aneurism.—In the article on *nævus* and on other occasions I have so fully detailed the operation of electrolysis, that I need only here refer to those points which are special to aneurism.

One difference between the use of the galvanic current in aneurism and in other tumours lies in the fact that, except for the prick of the needle, which may be avoided by cocaine or freezing, the operation is perfectly painless in the former, but requires an anæsthetic in the latter. In the one the electrodes lie in blood, in the other they are occupied also in cauterizing solid, and therefore more or less nerve texture.

The object aimed at is accordingly entirely distinct. In a tumour the intention is to destroy tissue, and especially the walls of vessels, and only in a subordinate and secondary way to produce an effect on the blood. In aneurism, on the other hand, coagulation of the blood is the aim of the operation, and the solid textures are carefully avoided lest evil follow. In the tumour the speedy absorption of the electrolytic products is desired; in aneurism it is hoped that the clot may be permanent, and even add spontaneously to its bulk.

It is easy to see, therefore, that electrolysis may be effective, useless, or dangerous in one and not in the other, that we cannot argue from its success or failure in erectile tumour to its success or failure in aneurism.

Electrolysis of blood leads to coagulation—first, by primary de-

composition ; next, by the secondary effect of the nascent products of decomposition ; and, thirdly, perhaps, by oxidation of the positive electrode. The character of the clot, therefore, will vary at the different poles. It is found, in short, by vivisection and other experiments, that there results at the positive pole a small, firm, black coagulum, at the negative a large, loose, but rather sticky white or pinkish froth, and around these a quantity of thin tar-like fluid in which the blood corpuscles are shrivelled, crenated, or altogether destroyed.

Now it is plain that the fluid portion of the electrolyte can be of no curative value but rather injurious, and that a useful result can be obtained only by the coagulum at the positive and the froth at the negative pole. Of these the former is durable but small ; the latter is large, but immediately after its formation begins to contract by absorption of the bells of hydrogen entangled in it.

It is evident from these considerations that an aneurism may be cured through electrolysis, if there be complete and immediate occupation of the sac by the solid products, if the fluids are expelled into the general blood stream, and if the aneurismal wall contracts in proportion to the shrinkage of the clot. Or alternatively, it may be cured because the artificial production of solid material provokes to the subsequent deposition of laminated fibrin.

But it is also evident that the blood stream may re-enter the sac by mingling with any fluid still unexpelled, or because the froth contracts more rapidly than the aneurismal wall ; or again, that if the blood be of non-coagulating quality, the solid matter introduced may fail to induce further deposition of clot or fibrin.

The results of my operations, now including a considerable number of aneurisms, illustrate and confirm these conclusions from experiment. And, first, electrolysis has not in my hands given rise to any untoward effects, so far as the electrolytic process itself is concerned. The evils that may be apprehended are the carriage into the general circulation of injurious electrolytic products, the production of hæmorrhage, and the excitement of inflammation.

So far as I can learn, the first of these possibilities has not happened in the hands of any surgeon. I suppose that the gases and liquids produced are not injurious, and that the solids are of such a character, and so adhere to the needles, and afterwards to the walls, that they are not easily carried away. It is indeed a comparatively rare event that portions of the normal clot become displaced, and perhaps the artificial products may be regarded as having similar probabilities. The chance is not to be left out of account, but is very unlikely to happen.

But hæmorrhage and inflammation have occurred, and have even produced fatal results in the operations of other surgeons. I attribute the complete immunity of my own cases to care in avoiding the electrolysis of solid textures, and in passing the needles through a part where the wall of the aneurism is of considerable thickness.

Three cases I have had in which there was a suggestion of these accidents, but without serious consequences, and from entirely preventable causes.

In one of my early cases guttapercha was used as the insulating medium, and a portion was stripped off in withdrawing the needle. I afterwards picked it out of the subcutaneous texture without evil result.

In yet another early case a small subcutaneous abscess formed. We were not then imbued with antiseptic principles, and I attribute it to the fact that our needles and fingers were not rendered aseptic.

In a third case, although there was no external hæmorrhage, considerable subcutaneous ecchymosis occurred. The pressure of the finger tips on the little punctures had not been sufficiently firm and prolonged to prevent the blood flowing from the orifices in the sac, or rather probably the pressure had been badly directed, because I endeavour to make the needle tracks somewhat valvular, and it is necessary to cover with the finger not only the external orifice but also the estimated situation of the internal.

But I have had neither external hæmorrhage nor inflammation of the sac, because I insulate with vulcanite the needles where they lie in the textures, and avoid touching anything but blood with the exposed points.

Only one risk, in short, ought to attend these operations. In two of my cases of aortic aneurism death by internal hæmorrhage occurred within a week after the operation. In one the opening took place into the pleura, in the other into the trachea. I do not know that the treatment and death are to be regarded as cause and effect, but it seems not improbable that the strengthening of the wall in one direction may have thrown greater pressure upon other points already little able to resist it.

But the great safety of the operation would be of comparatively little importance if the character of the blood products, as I have described them, should prevent electrolysis from having a permanent or satisfactory result, and it must be confessed that there is a tendency for the benefits derived to be merely temporary.

To obtain a fair idea of the value of electrolysis in aneurism it is necessary to have regard to the two varieties, internal and external. The operation has been performed far oftener in thoracic aneurism than in all others put together, partly no doubt from the greater frequency of internal aneurism, but partly also because the relation of electrolysis to other forms of treatment is very different in the two varieties.

Take first thoracic aneurism.

A not uncommon experience is the following:—The secondary sac of an aortic aneurism projecting through the chest-wall is rapidly enlarging and threatens external hæmorrhage. By the

end of the operation the forcible and expansile pulsation is reduced to a mere heave, and sometimes even that has next day almost disappeared. A certain diminution in size follows, but after a few days the pulsation becomes again more distinct, and if nothing be done the former condition is slowly resumed. In one case, indeed, this temporary effect was so evanescent that after three operations I thought it useless to persevere, and the patient ultimately died of external hæmorrhage. In it, however, the disease had advanced so far that a portion of the sac was already blue and thin, and it is precisely when the sac is incapable of contracting *pari passu* with the shrinkage of the clot that this merely temporary improvement is most often seen.

Per contra an exactly opposite result followed in a case where the wall of a secondary sac was already gangrenous over a space the size of a penny, and actually burst and emitted gas and froth during the operation. The swelling, larger than a fist, slowly disappeared, the pulsation never returned, and the patient died six months afterwards from empyæma produced by internal pressure.

We have then four results, which occur in my experience with different degrees of frequency.

1. The products are so soluble and evanescent that practically no benefit is produced by the operation.

2. The benefit, though only temporary, is yet of sufficient duration to enable us, by repeating the operation, to prevent increase of the portion of the tumour outside the thorax, and the threatened external hæmorrhage.

3. The external sac is entirely occluded, but the internal portion of the aneurism advances as before, it may be in some more rapidly.

4. Steady improvement sets in, and the whole aneurism diminishes in size, and sometimes even disappears.

Of these the first and fourth are rare, the second and third about equally common. I have had two cases of complete failure, and one in which apparent cure resulted, but I have had many in which impending death from hæmorrhage was averted, and the patient's life undoubtedly, and sometimes greatly prolonged.

If with these data in view one endeavours to assess the value of electrolysis in thoracic aneurism, it is proper to ascertain the position it ought to hold with regard to other remedies as well as the class of case to which it is applicable.

And, in the first place, no one can doubt that a thorough trial must be given to rest and other remedies which lower the frequency and force of the cardiac impulse. Failure assuredly would show that the resiliency of the aneurismal wall and the quality of the blood were such as to be unfavourable for any mode of treatment, but the safety and proved efficacy of these medical remedies assure them precedence.

The surgical remedies for thoracic aneurism are twofold:—(1.) The introduction of coagulants; and (2.) The ligature of arteries.

The ligature is necessarily distal, and therefore comparatively unsuccessful in any of its varieties or combinations. Perhaps that of the common carotid is as hopeful as any. In certain cases it will be found that pressure on the common carotid not only diminishes the force of the pulsation in the aneurism, but also the frequency of the cardiac action. Whether this may arise from diminished cerebral blood supply or from pressure on the vagus is a moot point, but it appears to me that we have here an indication of the class of case to which the treatment by ligature may legitimately be applied. In them I should prefer ligature to the introduction of coagulants.

The coagulants which may be employed are mechanical and chemical. Of the former, silver wire and horse hair have been proved liable to cause inflammation of the sac. Catgut I should be inclined to think safer, but its usefulness has yet to be gauged. I believe that no cure has yet resulted from any, although some improvement has been claimed. Of the chemical coagulants electrolysis is the only one which can safely be employed.

Given, therefore, cases in which a careful and prolonged trial of rest and other medical remedies have failed, and which do not appear suitable for ligature, and we have only electrolysis left.

Perhaps I should still further limit its application. I would hesitate to recommend electrolysis unless the aneurism had penetrated the thoracic wall, and external hæmorrhage were the death which seemed most likely. No doubt the most successful recorded cases of electrolysis are those in which the tumour has been intra-thoracic. So long as the resiliency of the sac is fairly preserved a comparatively slight strengthening of the wall may suffice to redress the balance between internal pressure and external resistance. But the very nature of aortic aneurism makes it absurd to expect that complete cure can often be effected by the production of a coagulum. Although I should not in intra-thoracic cases always refuse the chance, I should not readily abandon medicine, and I should require to assure myself that the needles could be safely lodged, with their exposed points entirely in fluid blood, before running the risk of inflammation of the sac from contact of the needles with the wall during an operation which afforded no great hope of permanent cure.

It is different with external aneurism. In them the surgical possibilities are so numerous, and the conditions are so varied, that the relative value of each method of treatment alters with the site.

We are perhaps too ready to abandon medical treatment in external aneurism. I had a most gratifying success in a fusiform aneurism in Hunter's canal, after ligature by another surgeon had failed, by rest with the leg elevated and the administration of iodide of potassium.

But putting these remedies aside, we have to choose between—

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| 1. Flexion. | 4. Ligature. |
| 2. Compression. | 5. The old operation. |
| 3. Coagulants. | 6. Amputation. |

1. Flexion, I take it, must be limited to popliteal aneurism, and only to those in which a moderate bending of the limb effectively diminishes the pulsation in the sac.

2. Like flexion, compression involves no breach of surface. It is safe and efficient. Before applying it we need only ask ourselves, Can the artery in this case be safely and satisfactorily compressed? Plainly there are mechanical difficulties which may render it impossible, situations in which other structures might be dangerously pressed upon, conditions of the circulation in the limb, or of inflammation of the sac which forbid its use. Otherwise it comes before all cutting operations or coagulants.

I cannot avoid recording here a very interesting case of popliteal aneurism, in which I used Dr Reid's application of the elastic bandage.

The patient, a man of fifty-five, was one day walking quietly down Leith Walk when he felt as if something gave way (doubtless it was the inner coats of the vessel), was seized with sudden and severe pain behind the knee, and with difficulty made his way home. Shortly afterwards he noticed a small pulsating swelling, which slowly increased, till, when he presented himself at the Infirmary, it had attained the size of an orange. The elastic bandage was applied on three occasions, and complete cure followed the last, of two hours' duration. On one of these occasions, my colleague Dr Miller, who had charge of the wards during my absence, kept the bandage applied for no less a period than six hours.

3. Next in order of sequence, as implying practically no breach of surface, come coagulants. There can be little doubt that electrolysis is the best of them. The possibility of judging by results during the progress of the operation gives it an advantage over the injection of perchloride of iron. In a traumatic aneurism of the radial I found this most useful. The operation was continued until the induration showed that enough had been accomplished, and one sitting effected a cure. In some cases the effect on the pulsation would also serve as a guide, but in this one, as an additional precaution, and because I thought it would aid coagulation, I had applied a tourniquet.

Reasoning from our knowledge of its action, and our as yet limited experience of it in external aneurism, I should say that there are three classes to which electrolysis may be usefully applied. There are, first, a few aneurisms of terminal arteries, such as the gluteal and sciatic, in which it appears singularly suitable. Compression is difficult, cutting operations are serious, while

electrolysis is likely to be successful, and from the situation is free from all risk of embolism.

Secondly, In cases where neither compression nor ligature can be applied with reasonable hope of success, and especially if the risks of embolism can be avoided by a distal tourniquet during the operation.

Thirdly, In cases to which compression has been applied but failed, it may well be used either alone or as an adjuvant during the repetition of compression, and before resorting to more serious measures.

These leave a considerable margin in which operative procedure must be undertaken, to which, of course, fall to be added those in which electrolysis may be unsuccessfully attempted. It is beyond the scope of this paper to discuss the particular applications of the various cutting operations.

Varix.—There is considerable difference of opinion as to what ought to be included under the term varix. Simple dilatation of veins over a rapidly growing tumour, varicocele, hæmorrhoids, and varix of the saphena, present very marked differences in their pathology, anatomy, and clinical history, and the forces at work in transforming a simple dilatation into one with inflammatory sequelæ are such that I think the term should be restricted to the latter.

They have all this feature in common—an alteration in the relation between internal pressure and external resistance. Their differences depend on the nature of the causes which bring about the alteration in pressure, on the anatomical structure, and on the external relations of the veins affected.

Let us take the saphena vein when varicose as presenting a typical form of the disease, and let us consider what happens on the assumption that it is produced by an increase in the pressure of the blood beyond what the walls of the vessel are calculated to bear.

The vein becomes dilated and elongated, and, because elongated, necessarily tortuous.

Liquid pressure acts equally in all directions. The elongating force, therefore, is less than the dilating, but becomes increased as the tortuosity gets greater. It is evident also that what with curves, valves, unequal thickness of wall, and varying density of surroundings, this pressure will meet with less resistance in some directions than in others. The dilatation therefore will necessarily be unequal, and so we have pouchings formed with stagnation and possible blood clottings and phlebolites.

But increased pressure in any portion of the body may, according to its severity and persistence, lead to atrophy, hypertrophy, or inflammation. If it be mild but continuous it produces atrophy, mild but intermittent it leads to hypertrophy, severe and continuous it induces atrophic, severe and intermittent hyper-

trophic inflammation. In varix we have each of these processes illustrated in different cases, or simultaneously in different portions of the same limb. In one part the vein and its surroundings may thin away till hæmorrhage takes place. In another its walls and the neighbouring parts thicken and contract from fibrous texture; while œdema, erythema, eczema, and ulceration may mark more advanced processes of inflammation.

If we inquire what are the causes of this alteration in pressure, it is plain that the action of gravity is a potent factor. It only can account for the greater frequency of varices in the leg, and of the tendency to varix on the part of those whose occupation implies prolonged standing. But other factors there must be, for this cause is universal. It does not account for the proneness to disease of the internal as compared with the posterior saphena, or of the superficial as compared with the deep veins of the leg.

Another cause evidently lies in venous obstruction as proved by the influence of pregnancy or other forms of external or internal pressure on venous trunks.

But all kinds of obstruction are not effective. If, for example, a vein be obliterated at a certain point it does not necessarily become varicose below. It is not uncommon to have the femoral vein obliterated in fever or from rheumatic or other causes to the extent of producing considerable and persistent œdema. But even those veins which take on the added current do not undergo the changes of varicosity.

That obstruction is most injurious which impedes, but does not entirely arrest the flow of blood. An interesting illustration of this, out of many, lies in the tendency which veins sometimes have to become varicose just below the point where they dip through a fascia, as at the saphena opening, or in the popliteal space. At both of these points, where the tenseness of the fascia causes a certain constriction or prevents free and equal enlargement, one may sometimes see large varicosity before its manifestations in other parts. There is here an analogy to the pouching of the urethra behind a stricture, or of the bowel or œsophagus under like conditions.

Again, it is essential that the pressure, obstructive or other, be of such a nature that it will continue to act, notwithstanding enlargement of the veins. When one vein is obstructed another dilates, but has no tendency to become varicose. Over the pregnant abdomen the veins grow large, but they do not pouch or become tortuous, or cause their surroundings to inflame, while if the veins of the leg suffer from pregnancy they do all of these things. The reason is plain,—the abdominal veins dilate because they have more blood to carry, but when they have dilated sufficiently to meet the demand, the pressure is of course again reduced to normal. But the veins of the leg dilate because there is obstruction, not because they have to carry more blood. The

pressure, therefore, remains increased, whatever be the size to which the veins have dilated. They enlarge because pressed upon, but the enlargement does not relieve the pressure as in the former case.

Even now, however, we have not arrived at a complete explanation of varicosity. Every washerwoman or pregnant woman does not become varicose. Plainly there is a factor in diminished external resistance. The want of muscular and fascial support, of course, supplies the explanation of the relative frequency of varicosity in the superficial as compared with the deep veins. But there is also, doubtless, in many instances a structural defect, a want of strength, muscularity, or elasticity in the wall of the vein itself. Varix is frequent in the young, and in those who have not been subjected to special strain, and is distinctly influenced by heredity independently of other circumstances.

With a clear view of the pathology the application of the recognised forms of treatment becomes easy. Two principles guide us—to diminish the internal pressure, and to strengthen the external resistance.

Of these the former is the more important, and too much neglected. Leaving aside the removal of all forms of obstruction as far as may be, there are very few cases of varicosity which cannot be reduced to manageable dimensions by absolute and prolonged recumbency with the leg elevated above the body. Thereafter the application of elastic support during the day, and elevation of the limb during the night will secure the comfort of the patient.

But there are a few cases in which the condition has been allowed to attain a large development, or in which the varicosity is very definitely limited to a single vein. In the former, the value of elastic pressure may be much increased, in the latter the normal state of the limb may be almost restored by obliteration of the diseased trunks. It has been urged that to do so is to throw a greater strain upon the other channels. But it has been already shown that a demand merely for conveyance of a larger quantity of blood is met easily by simple dilatation, and does not tend to produce varix. Practically, moreover, experience coincides with the true pathology.

There are many methods whereby this obliteration has been attempted. The only one which is definitely and assuredly successful is excision. But that operation is necessarily one of considerable severity, involving an incision and dissection many inches in length, and surgeons generally have preferred to resort to less severe remedies, especially in cases where many venous branches are involved.

The method of subcutaneous ligature by catgut which I introduced some years ago still appears to me the best, whether in varix or varicocele. A more extended experience has modified

very slightly the views I had adopted on the strength of my first few cases.

The catgut thread is passed first completely under the vein, and then by the same openings between it and the skin. After it has been knotted and cut short, the knot is buried by forcible traction on the skin. It is necessary that the catgut be strong enough to bear considerable strain, and so prepared that it shall remain unabsorbed for a considerable time.

In a large number of cases I have met with suppuration only twice. In one a ligature was placed on a vein over the patella, and the suppuration was caused by the patient's getting up and moving about. In the other, by some mischance I omitted to bury the knot and effectively cover it with wool and collodion. Fortunately, in neither did any harm result, but it is impossible to insist too strongly on the strictest antiseptic precautions in an operation dealing with veins. The last of these cases I have just seen, four years after the operation, on account of a small ulcer. The dilated condition of the minute venous radicles in the skin, which is always an important factor in the production of ulcer, remains, but the chief veins are entirely obliterated.

Although free from risk, ligature is liable to that failure which attaches to all operations which involve merely section of the vessel. A vein has a wonderful capacity for restoring its lost lumen. I endeavoured to provide against this, by advising that two ligatures be placed about an inch apart on each vein. For some time I found this quite satisfactory, but it is, I now know, not always absolutely reliable. I have of late, therefore, multiplied the number of stitches considerably, and so far have been pleased with the result.

I also advised, where the veins were numerous, that a careful study should be made of their anastomoses, and the ligatures placed accordingly. By multiplication of ligatures, I am now in this respect also more successful, but I am inclined to think that in a few cases it will continue to be necessary to perform the operation a second time on channels that may have escaped notice at first.

It is an operation always very easy in varicocoele, but in varix of the leg the amount of inflammation may sometimes make it difficult to draw the skin over the knot. A week's recumbency is sufficient to bring about a condition of texture sufficiently relaxed for the purpose.

When the scrotum is much elongated and relaxed, it may in some cases be advisable to remove a portion of skin. If that be so, the isolation of the veins from the cord and artery is of course greatly facilitated. It is not difficult to distinguish them in any case, but of course, when the sense of sight aids that of touch, precision is increased. I aim at including all the veins of the cord. The circulation is carried on by the superficial veins of the scrotum, which somewhat enlarge.

As I have already said, operative interference should not be undertaken unless other remedies fail, and the disease is a serious source of trouble to the patient. But when that is so, I have found this plan less severe than excision, and preferable in certainty and in safety to incision, external ligature, or coagulants. I have tried electrolysis, following Schuh, but have found a difficulty, both with it and with carbolic acid injection, in doing enough without doing too much.

Loss of Blood.—The relation of tension to growth and general nutrition forms one of the most far-reaching and interesting of physiological problems. It may be put generally that the body (possibly it is true of all organic things, or even all material substances) can only be maintained provided the constituents preserve a certain relative pressure, and conversely that the pressure necessary for nutrition is kept up by an elaborate system of adjustments, usually through the nervous system or the nervous qualities of all cells. In this relation Stricker's observation of the vital dilatation and contraction of gland cells is to the point. Other examples are to be found in the shortening of textures placed in a state of relaxation, in their lengthening when in tension, in the atrophy or growth of bone, according to increase or diminution of pressure, as in genu valgum or club-foot, in the projection of vascular loops and granulation tissue, when the cutaneous envelope is destroyed.

And so is it in the vascular system, where the relation of the blood to the vessels is maintained by a reflex adjustment of great nicety through the influence of the nerves. This balance between internal pressure and external resistance may be disturbed either by adding to or taking from the quantity of the circulating fluid, and some extremely instructive experiments have been made, which show the limits within which this may be done, and the great rapidity with which within these limits re-adjustment of tension takes place. Moreover, aberrations in the relation of blood to vessels may occur, not from addition to or subtraction of the circulating fluid, but also from disturbance of the nervous mechanism for re-adjustment.

These considerations are all involved in the problems so often submitted to the surgeon in connexion with shock and loss of blood.

The symptoms and dangers inherent in these states are due to the altered relation of the vessels to their contents. In both tension is reduced, but from different causes. In the former it is probable that the whole arterial system has its tonicity lessened by reflex paralysis, and that the heart, arteries, veins, and capillaries are alike affected. In the latter the reduced tension is brought about by actual diminution of the circulating fluid to a point below that which can be compensated by the contractile power of the vessels. In both secondary effects are doubtless

produced on and through the central nervous system by the circulatory change in it.

Putting aside the secondary results, there is a distinct resemblance in the symptoms and effects. In the one case the patient, as it were, bleeds into his abdominal veins, in the other externally. They markedly aggravate each other, and frequently it is extremely difficult to say how much of the patient's danger is due to shock, how much to hæmorrhage.

So far as treatment is concerned, it is plain that the indication in shock is to restore force to the cardiac impulse and increase the contractile power of the bloodvessels, although it is easy to understand that an increase in the amount of the circulating fluid might possibly at a critical moment temporarily restore a much-needed tension. The proposal to relieve the right side of the heart by bleeding leaves out of account the fact that the right side of the heart is full because it is paralyzed, not paralyzed because it is full.

In hæmorrhage, on the other hand, it is necessary to increase the quantity of fluid in the heart and vessels—to give them, as is sometimes said, something to contract upon, or, as it might be better put, something to provoke their contraction. But this very expression shows that benefit might be drawn from any stimulus to contraction such as is indicated for shock.

In both conditions, if not carried so far as to be incompatible with the continuance of the functions of the body, nature speedily restores the tension, in the one case by resumption of nerve action, in the other by absorption of fluid. But the after effects of blood loss in surgical injuries are more serious than those from shock. The factories for blood corpuscles are, no doubt, capable of meeting large demands in a short time. I have been often amazed at the rapidity of healing and of re-formation of blood in cases where there has been extensive hæmorrhage. I have been astonished, in the anæmic subjects of sepsis and fever, to see how quickly, when drainage or other treatment removes the fever, the colour returns to lips and cheeks. But if there be sepsis, and if there be fever, the reproduction of lost blood does not take place, and the patient is apt to go down rapidly from amyloid degeneration, or to succumb to one of the forms of septicæmia or pyæmia. In the warfare of the tissues with external organisms the former yield because they have been weakened.

One of the most interesting facts in connexion with ovariectomy is, that more patients are reduced to the lowest ebb and yet recover than after any other operation. I would attribute this to two causes—1st, That shock, an evanescent state, is in them, as a rule, a more important factor than hæmorrhage; and, 2nd, that if septic conditions do not at once obtain a footing they have little chance of doing so afterwards, from the certainty with which the wound can be protected.

In most other surgical operations hæmorrhage plays the more important part. This has long been recognised, and bloodless surgery is an established designation. It is not an absolute fact, for, notwithstanding precautions, there is generally more blood lost in an amputation than the traditional tablespoonful. Now that I catch the blood, I find it rare that two or three ounces do not slip away, and from six to ten is not an uncommon loss.

This is sufficiently serious when combined with shock, even at the first. At that time, as Ludwig's experiments also show, perhaps nearly as serious to the robust and healthy in proportion to their mass of blood as to the feeble subject of disease. Moreover, in the latter these operations are often performed through septic textures, and in the former the size of the wound makes asepsis difficult, so that the after effects which I have mentioned may readily come into action in both.

When we take all these considerations into account, it is plain that any plan which ensures the return into the vessels of the blood otherwise lost in a major operation must be valuable in saving many lives from imminent or more tardy death, and in materially hastening and ensuring ultimate recovery.

Doubtless the injection into the vessels of other fluids will meet some of the indications. Milk, defibrinated blood, and saline solutions have all been tried. But the first and second of them are plainly inapplicable to cases of emergency. It is necessary to have the milk freshly drawn, and valuable time may be lost in procuring, whipping, and straining the blood. They afford, moreover, at best an imperfect substitute for the fluid they replace, and if injection of blood as a whole be equally practicable, defibrination presents no comparative advantage whatever, and milk only in so far as a blood donor is not required.

I have on two occasions used saline solutions in patients who were in imminent danger of death from hæmorrhage. It is certain that a temporary rally takes place, but I have been unfortunate in finding it only temporary. Nevertheless, I believe it may be useful, and it is worthy of note that it is not incompatible with the method of reinjection which I recommend, and may be employed to supplement it by increasing the bulk of the fluid injected. Alone it cannot be regarded otherwise than as a means of temporarily averting death from diminished tension, and giving time for a possible rally.

What I propose is that the blood which would otherwise be lost in a major amputation or other great operation should be caught, and reinjected into any suitable vein in the wound or elsewhere. For this purpose it is necessary that means should be taken to keep the blood fluid for the necessary time. I believe that this may be best done by the addition to it of a solution of phosphate of soda. This mode of preventing coagulation was, so far as I know, first practised by Dr Braxton Hicks, on the recommendation of Dr Pavy, and I find it by experience simple and effective.

I have already published (*Brit. Med. Journal*, 30th January 1886) the details of the operation.

Take a case of amputation, primary or other. While the limb is being amputated and the arteries tied, the blood is caught in a dish containing a 5 per cent. solution of phosphate of soda in distilled water. The solution is further added, as may be required, to keep the proportion of 1 to 3 of blood, which may, of course, be easily done by having the dish graduated. Care should be taken that any error of quantity shall be on the side of the soda solution. A small piece of glass tube, conveniently made pen-shaped, is tied into a vein on the face of the stump. To this the syringe is attached, and the fluid is very slowly injected.

It is not difficult for one person to manage the whole process, but it is convenient that an assistant shall catch the blood and mix with it the soda, another fill and refill the syringe, and a third inject the blood and pinch the indiarubber tubing while the syringe is being refilled. The injection thus goes on while the operator is tying the vessels, and in that way the patient is not kept longer on the table than he otherwise would be.

One or two points of detail, of course, require attention.

The syringe should be held above the horizontal, that air may not be injected.

It is plainly for the good of the patient that the solution be thrown in as nearly at blood heat as possible.

The most rigid antiseptic precautions must be taken with fingers, instruments, and dishes, but no spray or antiseptic solution can be applied to the wound while the blood is being caught. In this view it is obvious that in operating through septic tissues the utmost caution must be exercised, and I myself rather amputate an inch or two higher than run this risk.

In no case in which I have injected blood, and they are now numerous, has there been that rigor or rise of temperature which has marked other methods. In one case of compound fracture the patient was brought in wildly delirious from delirium-tremens, and the temperature followed the course of that disease and of the sepsis of a knocked-about knee-joint amputation. But nothing marked the reinjection of his blood, and he ultimately got perfectly well. I have been fortunate in having no fever with my other cases.

What may be the cause of this superiority over other methods it is impossible definitely to determine at present. It must be one of two things, and it may be that either will produce the rigor—viz., the introduction of septic matter, or embolism from incipient coagulation.

When one considers the high but evanescent fever that often accompanies septic infection, I find it difficult to avoid the conclusion that this is the cause. How immediately does the highest fever often cease when efficient drainage of a wound is established,

how severe yet evanescent is that which may follow catheterization or the passage of septic urine over a raw surface.

On the other hand, saline injections are little prone to be followed by fever, defibrinated blood is often free from it, while milk and pure blood without soda apparently determine it with great certainty. This is not incompatible with the septic hypothesis, but points more directly to the embolic. I have been scrupulously careful to avoid both sources of evil, and to this I attribute the freedom from rigor or fever. I am well aware that a certain number of septic organisms may be introduced into the blood without observable damage, that to a given extent it is a matter of quantity. It is easy to understand that up to a certain point the blood may be able to destroy the intrusion without giving indications of its presence, but that a larger quantity implies a feverish struggle, and a still larger determines multiplication and final predominance of the poison. My fear of the future of the operation lies in a neglect of the stringent precautions needful to avoid the contingencies of septicity and embolism, and lest it should fall into discredit from too wide an application, I advocate it as perfectly safe, and capable of saving many lives in the major operations of surgery. I make it a routine practice in all the larger amputations, because there is no risk, and every ounce of blood is serviceable. What the value of the method may be as a means of transfusing blood in disease is yet quite undetermined, and is, indeed, only to be discovered by careful experiment in each. Other methods of maintaining blood in a fluid state may yet be found more advantageous in practice, but to my own mind the principle of reinfusing the patient's is now definitely established.

II.—CASE OF INTERNAL STRANGULATION.

A portion of a Clinical Lecture by JOSEPH BELL, Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery, Edinburgh.

ON my arrival at Hospital on Friday morning I found that there had been admitted a patient who was said to be suffering from strangulated hernia. On going to his bedside I found an old man between 70 and 80, looking very much exhausted, lying in the position of dorsal decubitus, with a pulse hardly to be felt, his complexion pallid, and his skin covered with an unwholesome, clammy sweat. His abdomen was not much swollen. In the left groin, just above Poupart's ligament, was a small tumour about the size of a large pigeon's egg flattened, which, however, had no appearance of inflammation nor of tension, and had no impulse on coughing. This was the difficult and troublesome point of the case. On inquiring into the history, I with difficulty discovered that for seventeen days he had had no proper motion of the bowels, that the

tumour had been present for that time, having come on after a strain, that he had been vomiting a great part of the time, and that he had vomited worse during the last three days. The doctor had not been called in till the night before, and had at once recommended his removal to the Infirmary. His friends neglected to do so till the following morning, and when they did, brought him in only half-clad and without his boots. He was so nearly moribund, and the case presented so many difficulties, that the question arose whether operative interference was advisable. Acting, however, on the principle that no hernia should be left unoperated on, I placed him on the table, though I felt that we might not get him off it alive. I then cut down upon the tumour, but after exposing the sac could feel no bowel in it. On opening it a piece of fat extruded. This I unravell'd, and exposed a bit of very hard fatty and fibrous tissue. We determined to examine this, and, cutting down layer by layer, came upon a small central cavity, out of which appeared a quantity of putrid pus. I then thought it right to expose the internal ring by slitting up the tissues of the canal upon my forefinger. Entangled in the ring we found a small piece of bowel still alive, but inflamed, and adherent to the abdominal wall. On examining further, I found that the loops of intestine in the neighbourhood were glued together by lymph, peritonitis having taken place. A band had actually formed, binding one coil of intestine to another down in the space between the bladder and the rectum. This I was able to set free, the band giving way under my finger. Next came the question whether we should make an artificial anus or give the bowel the chance of living and return it to the abdomen, putting in a drainage-tube. I did the latter, fearing that the making of an artificial anus would very quickly prove fatal. Though he had been so ill, he was improved by the operation. He died in twenty-four hours of exhaustion, but during that time had an easy movement of the bowels, and did not have any more vomiting. Had we seen him three or four days earlier, we might have saved his life.

III.—A CLASSICAL REMEDY.

By G. A. GIBSON, M.D.

No doubt many of my professional brethren are well acquainted with the Hippocratic aphorism,¹ Ὑπὸ λυγμοῦ ἐχομένῳ πταρμοὶ ἐπιγενόμενοι λύουσι τὸν λυγμόν, "Sneezing occurring after hiccup removes the hiccup," or, as it is rendered by Adams,² "Sneezing coming on, in the case of a person afflicted with hiccup,

¹ Littre, *Œuvres Complètes d'Hippocrate*, tome quatrième, p. 566. Paris: Baillière, 1844.

² Adams, *The Genuine Works of Hippocrates*, vol. ii. p. 754. London: Sydenham Society, 1849.

removes the hiccup." Familiar as I am with the aphorism, it never seemed to me that it might be made of use in the practice of our art until quite lately; and, as others also may not have thought of making any practical application of the observation, I venture to bring it before them anew. When devoting a leisure hour, as I sometimes do, to Plato's Dialogues, as translated by Jowett, I was struck by a passage in the Symposium which had never arrested my attention before. Although perfectly satisfied with the translation referred to, as the medium through which the greatest thoughts of the Hellenic mind may reach one who is not a Grecian, I have looked up the original, which runs as follows¹:—

Παυσανίου δὲ παυσαμένου, διδάσκουσι γάρ με ἴσα λέγειν οὕτως· οἱ σοφοί, ἔφη ὁ Ἀριστοδήμος δεῖν μὲν Ἀριστοφάνη λέγειν, τυχεῖν δ' αὐτῷ τινα ἢ ὑπὸ πλησμονῆς ἢ ὑπὸ τινος ἄλλου λυγγα ἐπιπετωκίαν καὶ οὐχ οἷόν τ' εἶναι λέγειν, ἀλλ' εἰπεῖν αὐτόν—ἐν τῇ κάτω γὰρ αὐτοῦ τὸν ἱατρὸν Ἐρυξίμαχον κατακεῖσθαι—ὦ Ἐρυξίμαχε, δίκαιος εἰ ἢ παῦσαι με τῆς λυγγὸς ἢ λέγειν ὑπὲρ ἐμοῦ, ἕως ἂν ἐγὼ παύσωμαι. Καὶ τὸν Ἐρυξίμαχον εἰπεῖν, Ἀλλὰ ποιήσω ἀμφοτέρα ταῦτα. ἐγὼ μὲν γὰρ ἐρῶ ἐν τῷ σῶ μέρει, σὺ δ' ἐπειδὴν παύσῃ, ἐν τῷ ἐμῷ· ἐν ᾧ δ' ἂν ἐγὼ λέγω, ἂν μὲν σοι ἐθέλῃ ἀπνευστὶ ἔχοντι πολὺν χρόνον παύεσθαι ἢ λύγξ· εἰ δὲ μὴ, ὕδατι ἀνακογχυλίασον. εἰ δ' ἄρα πάνν ισχυρά ἐστιν, ἀναλαβὼν τι τοιοῦτον, οἷω κνήσαι· ἂν τὴν ῥίνα, πτάρε· κἀν τοῦτο ποιήσῃς ἀπαξ ἢ δις, καὶ εἰ πάνν ισχυρά ἐστι, παύσεται. Οὐκ ἂν φθάνοις λέγων, φάναι τὸν Ἀριστοφάνη· ἐγὼ δὲ ταῦτα ποιήσω.

Translated by Jowett the foregoing passage stands thus²:—
“When Pausanias came to a pause (this is the balanced way in which I have been taught by the wise to speak), Aristodemus said that the turn of Aristophanes was next, but that either he had eaten too much, or from some other cause he had the hiccough, and was obliged to change with Eryximachus, the physician, who was reclining on the couch below him. Eryximachus, he said, you ought either to stop my hiccough, or to speak in my turn until I am better.

“I will do both, said Eryximachus: I will speak in your turn, and do you speak in mine; and while I am speaking let me recommend you to hold your breath, and if this fails, then to gargle with a little water; and if the hiccough still continues, tickle your nose with something and sneeze; and if you sneeze once or twice, even the most violent hiccough is sure to go. In the meantime I will take your turn, and you shall take mine. I will do as you prescribe, said Aristophanes, and now get on.”

As may be seen in the sequel, the hiccough was not cured by the first or by the second remedy suggested by Eryximachus, but by the production of sneezing.

¹ Hirschig, *Platonis Opera*, volumen primum, 668. Parisiis: Didot, 1856.

² Jowett, *The Dialogues of Plato*, vol. i. p. 502. Oxford: Clarendon Press, 1871.

This part of the dialogue recalled to my mind the aphorism which has been quoted, and I have brought it under the notice of some of my friends. The method of tickling the nostrils has been tested by us in cases of obstinate hiccough, and has been very successful. Hiccough, as well as sneezing, is one of the specially modified respiratory movements, and it is quite in accordance with what we know of the transference of nervous action, that the spasmodic contractions of the diaphragm should cease on the induction of the explosive expirations which constitute the acts of sneezing. There is one point, however, which deserves special mention. It is not necessary that the stimulus applied to the nose be followed by sneezing; the application of a gentle irritant to the nasal mucous membrane may be quite enough to put a stop to the hiccough, by diverting the nervous energy into other channels, although it may not be of sufficient power to induce sneezing.

IV.—REMARKS ON HERNIA, WITH EIGHTEEN CASES OF OPERATION.

By H. A. LEDIARD, M.D. Ed., F.R.C.S. Eng., Surgeon to the Cumberland Infirmary.

(Read before the Carlisle Medical Society, 10th December 1885.)

AS no age, sex, country, or disease is exempt from rupture, there can be no necessity to plead for the importance of a subject so full of general interest; for whether we take the writings of the past or the surgical history of our own times, it is everywhere evident that hernia has always commanded a large share of attention.

I scarcely know whether the strangulated forms of rupture have interested surgeons more than that class which have been the subject of attempts to cure radically, but it seems to me that in the present day, at any rate, attention is more directed to the latter class than the former.

In the history of radical cure I should maintain that Sir Joseph Lister has, indirectly, gone far towards revolutionizing our ideas. I say indirectly, because I believe that it is owing to the bold spirit of inquiry which he brought to bear upon the treatment of wounds, that we to-day are found fearlessly undertaking the surgical treatment of a class of cases hitherto relegated to the instrument maker. I am of opinion that the surgical treatment of hernia has received a very important impetus during the last few years; and that whereas, say ten years ago, Mr Wood was about the only man in this country curing hernia radically, we now find that on all sides there are surgeons at work trying new operations in this direction, with a freedom unknown in prehistoric days. At the same time I am disposed to believe that the statistics

of strangulated hernia will be found to participate in the benefit of the movement, for it seems natural that surgeons who have been taught how to respect the peritoneum by attention to cleanliness will in all probability be less inclined to trust to depleting measures, baths, violent efforts at taxis, waiting, ice, and opium, when they feel confident that a careful incision made at an early date is scarcely associated with risk, whilst the results obtained from early operative interference will undoubtedly justify any outcry at rashness.

For years it seems to me that the text-books have been pointing the way to early interference by the surgeon, showing in a simple way how proportionate are deaths in operation to the delay that has taken place. I now believe that early interference, instead of late, will be the leading feature of the treatment of strangulated hernia from henceforth.

In dealing with this subject I fall back upon the scanty gleanings of five short years since I came to Cumberland; but I trust that these cases will serve as a peg upon which to hang a few remarks which may lead to expressions of opinion from those present, for all here have had to do with hernia in one or more of its many forms.

I first take Strangulated Hernia. Of these I have had nine cases from hospital and private to draw from, and here at once I seem to come down upon the truth of one of my first observations, for of the three operated upon in private practice all recovered, whereas of the six treated in hospital three died. Where does the difference lie? Simply in the delay before operation in the latter class, for I have no hesitation in saying that had those three cases ending fatally been operated upon in due time, there was nothing in the nature of the hernias to prevent recovery. In private practice there is no reasonable loss of time. The doctor is called; he at once applies taxis; he fails to reduce, but immediately proceeds to have his case operated upon, with the result that the patient's bowel is scarcely injured, either by prolonged handling or pressure at the constricting point. Take the other side. A hospital patient may have difficulty in getting medical aid with promptitude. Ignorance may blind him as to the cause of his sickness and colic; he may live miles away in the country; sooner or later, however, the doctor comes; taxis is applied; perhaps a long journey may be undertaken to fetch chloroform, further medical aid, and instruments, entailing delay; perhaps the patient is sent to hospital, and again subjected to manipulation by the house-surgeon, and again, when the surgeon arrives, handling which the bowel resents more and more according to the lapse of time since strangulation set in; perhaps the bowel is nipped beyond recovery, and if returned or left the result may be the death of the patient from delay, and delay only.

I take up this strong position because I do not think it has

been sufficiently insisted upon. I would rather too many herniotomies were performed than have to make the complaint that can be justly made to-day.

Of the three hernias which were successful, all were femoral—two being females and the third an old man of 73. The two first attended to presented nothing remarkable; they had been ill thirty-six and twenty-four hours respectively; in one the sac was opened, and in the other it was not. Both were treated with rigid Listerism. The third case, that of the old man, the hernia had been strangulated twenty-nine hours, and he was remarkably collapsed prior to operation. He was dressed with some lint and a bandage, and did well.

I now take the hospital cases, of which three died and three recovered. Of the recoveries the duration of the hernias was twenty-four, forty-eight, and less than twenty-four hours respectively. Here again a reasonable delay only occurred, although two came a distance of over twenty miles. One was femoral, in a woman; the other two were inguinal. The femoral sac was ligatured and cut away, the operation being on 9th April 1881, strict antiseptics being used.

Of the inguinal recoveries one was strangulated at the internal opening of the canal, and was associated with a hydrocele; a rather extensive incision was necessary to reach the point of constriction, and union, owing to the condition of the tunica vaginalis, in some measure was slow, and by granulation.

The lad is still a postman at the general office in Carlisle. The date of his operation was May 22, 1881.

The third case was that of a child of 18 months, with an infantile hernia. The sac was opened, and the contents consisted of the cæcum and vermiform appendix. There was great difficulty with the antiseptic treatment on account of the frequent urination.

I now pass on to the deaths.—The first was a youth of 21, who came a long distance, and had a strangulated inguinal hernia of eight days' duration. On incising the scrotum, the sac, and bowel, and omentum were in a state of advanced gangrene. He was well advanced in peritoneal inflammation, as was definitely proved at the post-mortem not many hours after operation.

The second death was also in a lad of 20, whose hernia (inguinal) had existed three days. The sac was opened; the bowel was doubtful and dusky, but, on account of his youth, I gave it a chance. The post-mortem showed that the gut was in process of sloughing, and had originated a peritonitis. The bowel was, however, secured to the abdominal wall by recent adhesions. Leaving the gut *in situ* at the time of operation might have saved this patient's life.

The third death was that of an old man of 75. A strangulated entero-epiplocele had existed five days; he was sent into hospital at the end of four days, and we were certainly somewhat slow in operating. The case, however, was not a clear one, for the chief

mass of the rupture was omental, and at the operation this was all that we found; it was ligatured and cut away, the stump being allowed to retract into the abdomen. Sudden death, twenty-four hours after, enabled us to see that the rupture had originally consisted of a bit of bowel wrapped up in omentum, and either prior or subsequent to admission, the bowel had been reduced, but remained, after reduction of the omentum, in a nipped condition inside the tapering fold of the omentum, but without constriction of any kind. The nipped portion gave way, and fæcal extravasation caused death. I maintain that all these fatal cases, followed to the end by post-mortem examination, are to be classed as too late cases. I am not wishful to cast blame on any one, I simply note the fact. So much for the nine strangulated hernias operated upon in Cumberland.

The cases which strike me as more curious than the others are the strangulated incomplete hernia, with hydrocele, in the postman, and the contents of the sac in the child, consisting of caput cæcum, and vermiform appendix. This variety is more likely to be met with in infancy and childhood, as these parts are late in taking up their permanent position in the abdomen. The shock of the old man with femoral hernia was remarkable, and no doubt attributable to his age, for he looked older than he was.

I now pass on to the cases of the other class, viz., those hernias where radical cure has been performed with more or less success. I have no cases to draw from in private practice, for hernia amongst the upper classes of society is as infrequent as prolapsed uterus is common amongst the women of the labouring orders, hence my nine cases are all hospital cases. Seven men and two women were submitted to operation. The first two operated upon were boys, aged 9 and 5. Wood's operation was performed. In neither was a truss of the least value prior to operation, but of enormous value afterwards, for the apertures were too large to be closed absolutely, and trusses were worn, and worn with comfort and benefit after operation, whereas before they had been discarded.

I showed these two boys at a meeting of the Border Counties Branch in June last. One boy had not worn a truss for a year, and there was a tendency to hernia which a truss controlled; the other boy was absolutely cured, but wore a truss for protection by my wish. One other Wood's operation I did, on a man with a hernia which had not reached the scrotum, in fact it was an incomplete hernia; the wire caused such an amount of suppuration that no good resulted, and as far as I know the operation was a failure, but I never saw him after he left the hospital, as he was a sailor from West Cumberland; my belief is that I failed to do any good.

I take the two women next. One was a small femoral hernia, of eight years' standing, which caused the girl pain and difficulty in following her employment as domestic servant; a truss had been tried ineffectually. I cut down, ligatured the sac, and cut it away.

She did well, although she was pregnant and suffering from secondary syphilis, which came out as surprises in the course of the treatment subsequent to operation. Her case was successful.

The other female's case was an umbilical hernia of six years' duration; the bowel was reducible, but the omental portion of the hernia was adherent to the sac. The skin, sac, and omentum were cut away, and the orifice in the abdominal wall was plugged with the stump of the omentum, and under wood-wool dressing she quickly went out quite well. According to anticipation, she became pregnant; I attended her in her confinement, and found that the adhesions were insufficient to bear the strain of pregnancy, and that the hernia was almost as bad as ever.

The remaining four cases were aged 43, 63, 45, and 3 years respectively. The first was a large irreducible omental scrotal hernia. I cut away the sac and omentum, the operation being on September 25, 1882. A good result was obtained.

The next one was a small femoral omental hernia, and the sac and omentum were cut away. He also did well.

The third was a reducible inguinal hernia. The sac and omentum were cut away, and the stumps ligatured to the pillars of the ring. A good result was obtained.

The last case is that of a boy of three years, with a congenital scrotal rupture. Here I cut down upon the tumour, avoided opening the tunica vaginalis, and tied the pillars of the ring together with catgut; a good deal of suppuration has resulted, and we have to depend upon the adhesions resulting therefrom to cause obliteration of the tunica vaginalis at the external abdominal ring. It is too early to say what success may attend this operation. The boy is at present in hospital. (Discharged cured, 8th July 1885.)

I admit that these cases are not sufficient in number, length of time since operation, or record since operation, to be of much value, but they are all I have of any individual performance to lay before you to-night. I have not tried Mr Spanton's operation, and am not very much struck with his instruments for the operation, or the results obtained by those who have followed his plan, hence I am quite unable to say a word for or against the method proposed. I am furthermore not particularly interested in naming the surgeon who has either advanced new methods of treatment, or who claims the greatest share of surgical *kudos* in the performance of radical cures, and I shall mention facts that have come under my own knowledge.

When I was the late Professor Spence's house-surgeon, I remember lending a hand one afternoon to Mr Joseph Bell, who on the same day had to deal with two strangulated hernias,—in one case a thick old sac was left after reduction of the hernia by operation, he cut away the sac and tied the mouth. I believe the case did well, and remember his coming to Mr Spence's room to relate this occurrence nearly fifteen years ago.

In the year 1870 I witnessed Sir Joseph Lister operate on an old irreducible umbilical hernia in the theatre of the Edinburgh Infirmary. The case is well imprinted upon my mind, because the dressing was lac plaster, the last material used before gauze was thought of and perfected. I think it only fair to mention these cases operated on by Mr Bell and Mr Lister, because it seems to me that their claims to recognition as early operators in the field for the radical cure for hernia have not been mentioned.

Before concluding my remarks, I wish to draw attention to a specimen of hernia reduced *en masse* by a patient. The man suffered from phthisis, and in the last stage almost of the disease came under my care for vomiting. He said he had had a rupture for some years, but had that day put it back himself, as he had done before. I found that there was no hernial protrusion, but was somewhat uncertain as to the satisfactory reduction of the hernia, because he continued to vomit and ultimately died. I was not over and above keen as to interference on account of his chest, which, together with the vomiting, proved fatal. I found the specimen which I now show you to-night.

The reduction *en masse* of a hernia by a patient is probably of very rare occurrence.

NO.	NAME AND DATE.	DESCRIPTION OF HERNIA.	RESULT.	REMARKS.
1	Miss G., age 39. 27th Sept. 1880.	Strangulated Femoral Hernia (left side).	R.	Strangulation, 36 hours. Herniotomy. Sac opened. Strict antiseptics.
2	Mrs B., age 39. 8th Nov. 1880.	Strangulated Femoral Hernia (right side).	R.	Strangulation, 24 hours. Herniotomy. Sac not opened. Strict antiseptics.
3	Janet C., age 40. 9th April 1881. Cook (Hospital).	Strangulated Femoral Hernia (right side).	R.	Hernia, six years' duration. Strangulation, 24 hours. Herniotomy. Sac not opened, but ligatured and cut away. Strict antiseptics.
4	Jacob G., age 31. 22nd May 1881. Postman (Hospital).	Strangulated Inguinal Bubonocoele (right side), associated with Hydro- coele.	R.	Hernia, five years' duration. Strangulation, 48 hours. Herniotomy. Sac opened. Gut strangulated at inner abdominal ring.
5	William L., age 73. 26th July 1883.	Strangulated Femoral Hernia (right side).	R.	Strangulation, 29 hours. Herniotomy. Sac not opened. Extreme collapse. Dry dressing.
6	James R., age 31. 16th April 1884. (Hospital).	Strangulated Inguinal Hernia (right side).	D.	Strangulation, 8 days. Herniotomy. Sac opened. Bowel gangrenous, and left, omentum cut away.
7	Daniel F., age 20. 12th June 1885. (Hospital).	Strangulated Inguinal Hernia (left side).	D.	Strangulation, 3 days' duration. Herniotomy. Sac opened. Bowel doubtful and dusky, but returned.
8	R. S., age 18 months. 18th June 1885. (Hospital).	Strangulated Right Inguinal Hernia (infantile).	R.	Strangulation. Herniotomy. Sac opened. Protrusion formed of cæcum and vermiform appendix. Sac ligatured.
9	George C., age 75. 13th Nov. 1885. (Hospital).	Strangulated Left Femoral Hernia.	D.	Strangulation, 5 days' duration. Herniotomy. Sac opened. Only omentum found, and cut away.

RADICAL CURES.

NO.	NAME AND DATE.	DESCRIPTION OF HERNIA.	RESULT.	REMARKS.
1	Arthur G. L., age 9. 1st Oct. 1890. (Hospital).	Left Inguinal Hernia from birth. Trusses no use.	R.	Wood's operation. Shown 26th June 1895 at Branch meeting.
2	Robert E., age 5. Feb. 1891. (Hospital).	Left Inguinal Hernia ac- quired during an attack of Whooping-Cough.	R.	Wood's operation. Shown 26th June 1895. Had worn no truss for 12 months. Tendency to relapse.
3	James A., age 43. 25th Sept. 1893. (Hospital).	Left Inguinal Hernia, irreducible.	R.	Herniotomy. Large portion of omentum cut away, and stump stitched to pillars of ring. Strict antiseptics.
4	George J., age 63. 14th Dec. 1893. (Hospital).	Small Femoral Hernia, Omental (on right side).	R.	Somemonths' duration. Tumour, doubtful. Herniotomy. Omen- tum cut away. Sac ligatured. Strict antiseptics.
5	Martha E., age 27. 4th April 1894. (Hospital).	Small Femoral Hernia of eight years' standing.	R.	Herniotomy. Sac ligatured and cut away. Strict antiseptics.
6	William A., age 45. 1st Sept. 1894. (Hospital).	Right Inguinal Hernia, reducible. Eight years.	R.	Herniotomy. Sac and omentum cut away. Stump ligatured to pillars of ring.
7	Mrs F., age 29. 9th July 1894.	Large Umbilical Hernia. Six years.	R.	Omentum, sac, and redundant skin cut away. Wood wool dressing.
8	William G., age 3. 20th Nov. 1895. (Hospital).	Inguinal Hernia—Con- genital (right).		Herniotomy. Sac not opened. Pillars of ring drawn together with catgut.
9	Name not noted. Date not noted. Age about 32. (Hospital).	Right Inguinal Hernia. Bubonocoele only.	R.	Wood's operation. Considerable suppuration, and result doubtful.

V.—A NOTE ON SCARLATINAL ALBUMINURIA.

By HENRY BARNES, M.D., F.R.S.E., Physician to the Cumberland Infirmary.

(Read before the Carlisle Medical Society, 10th December 1895.)

It would appear from observations that have been made by Haidenheim and Frerichs, that albuminuria is more frequently met with in some epidemics of scarlet fever than in others; and my experience of the late epidemic which has visited this district is confirmatory of this view, as I have certainly found it in a larger proportion of my cases than formerly. Sometimes it has appeared as early as the third day of the disease, and often before the end of the first week. It is necessary that frequent examinations of the urine should be made in order to appreciate its clinical significance. In one case the albuminuria was a help to a correct diagnosis. A patient had been exposed to the contagion of the scarlet fever poison, got a characteristic sore throat with pyrexia, but no distinct rash. He was very averse to believe that he had got scarlet fever, and the measures of isolation which I urged were very imperfectly carried out. The appearance of albuminuria in the case strengthened my suspicions as to the nature of the illness, and the

subsequent occurrence of other cases of undoubted scarlet fever in the same house placed the diagnosis beyond question. Along with the albuminuria during the pyrexial stage there is usually a large quantity of mucus with abundance of epithelial cells, due apparently to a catarrhal condition of the renal tubules. I have seldom found blood cells at this stage; but when desquamation has set in, then both blood cells and casts may be found, showing that a true nephritis has been set up. In the early period of the disease I look upon the albuminuria when it occurs in small quantities as due to hyperæmia; but when it persists for some weeks after the pyrexia stage has passed, a careful examination will usually reveal the presence of casts. In some cases I have found the albuminuria to be remittent in character. In one case which I observed very closely, the albumen appeared in the urine on the third day, when the temperature was 101°. The urine was tested twice daily, and always showed the presence of albumen until the fifteenth day, five days after the pyrexia had subsided. From this to the twentieth day albumen was absent in the morning urine, but present in the evening. On the twenty-first it was absent both in the morning and evening. On the twenty-second it was absent in the morning, but present at night; and traces could be found on five subsequent days, sometimes in the morning and sometimes in the evening. It was further noted that its presence appeared to bear some relation to the ingesta. If the diet had been highly albuminous, such as eggs or beef-tea, I usually found albumen in the urine. If, on the other hand, the patient had been kept on a milk diet, there was no albuminuria. During the whole period the patient was carefully nursed and kept in bed.

The lesson which this case has taught me is, that it is necessary the urine should be frequently tested in scarlatinal patients before the patient is pronounced to be convalescent. The urine should be examined both in the morning and evening for several days in cases where albuminuria has occurred, and no patient can be said to be cured if the kidneys are so far disabled that they permit the transudation of albumen when the patient is allowed an albuminous diet. I have seen some cases of incurable kidney disease which could be traced back to an attack of scarlet fever several years before, and therefore I think it of importance to keep a careful watch upon cases of scarlatinal albuminuria. In the way of treatment, I look upon rest in bed and a milk diet as of the greatest importance.

VI.—THERAPEUTIC NOTES.

By J. LINDSAY PORTEOUS, M.D., F.R.C.S. ED.

OF late there has been a great influx of new drugs, some of great value, others of little or no use. Where a medical man has

an extensive practice, consisting of rural and urban patients, he has ample opportunity of testing the effects of drugs, as the varieties of disease that come under his notice are great; and although his means of watching the actions of drugs are not so good as in hospital practice, yet a good deal can be done if he cares to take a little trouble to "take notes."

The following list of medicines, more or less new, have been used for some time by my colleague (Dr Proudfoot) and myself, and I give the results:—

Bromidia.—About eighteen months ago a friend of mine from America told me of the wonderful effects of a medicine, much used in the States, called *Bromidia*. According to the makers, it is composed of chloral hydrate, 15 gr.; potassium bromide, 15 gr.; extract of cannabis indica, $\frac{1}{2}$ gr.; and extract of hyoscyamus, $\frac{1}{2}$ gr. I obtained some, and have ordered it regularly for over a year; and have found it excellent in the pain of rheumatism, pneumonia, and cancer; also in the sleeplessness of scarlatina and alcoholism. It has never failed me in procuring sleep, without the disagreeable dreams and after-effects of opium. The dose is 3ss. to 3j. every hour till sleep is procured. I have also found it of much service in cases of tonsillitis, used as a gargle with glycerine and carbolic acid.

Grindelia Robusta.—This, in the form of a tincture, is an excellent tonic expectorant, especially in chronic bronchitis. It is also useful in pneumonia, and almost a specific in asthma. Along with *Quebracho* it greatly relieves the patient *during* the attack, and lengthens the period between the attacks. The use of iodide of potassium and liquor arsenicalis, vaunted so much by some medical men as a *cure* for this malady, does, in my opinion, more harm than good.

Tincture of Quebracho is very serviceable in the emphysema of pleurisy. It seems to have a decided power to lessen the respirations and dyspnoea. It is said to be beneficial in phthisis and mitral regurgitation, but of this I have had no experience. The relief it affords in dyspnoea is supposed to be due to its power to increase the strength of respiration, thus facilitating the supply of oxygen. I generally give 15℥ or 20℥ for a dose.

Tincture of Coto, made from the bark of a tree growing, I believe, in Bolivia and Brazil, I have found an excellent remedy in some forms of diarrhoea, especially in children. The only objection to its use is that it is rather hot, and should be well diluted.

Oleum Deelinæ.—My attention was drawn to this oil last June by an article in a medical paper by Dr Roberts of Chester. I wrote to him, and he kindly sent me some to try. It is a pure amber-coloured oil, without smell, and with little taste. It seems to evaporate or be absorbed very rapidly, as in a very short time after it is applied to an exposed surface, there is no trace of it. My experience of it is very favourable. In one case of gouty

eczema of ten years' standing (acting up to Dr Roberts' instructions), I had the parts affected washed well with oatmeal water, then the oil was gently smeared over the surface, and a piece of soft cotton applied. This treatment was continued once a day for ten days, when the eczema entirely disappeared.

In another case, one of sycosis, of three years' duration, which had been treated by many medical men and quacks, the success attending its application was surprising. An attempt had been made to extract the hairs of whiskers and beard, but the pain was so great that the man refused to allow it to be continued; and in July consulted my colleague, Dr Proudfoot. He ordered *Oleum Deelinæ*, to be rubbed well in, night and morning for a week. At the end of this period we found that suppuration had taken place at the roots of the hairs; and on applying the depilatory forceps, we found that the hairs came out without any pain. In a few days the parts became quite healthy, and remain so still. It is difficult in this case to say whether it was the depilation that cured or the oil; but if the oil did not cure directly it did so indirectly, by loosening the hairs and making the operation of extracting them painless. In the "scalding" of infants it is very beneficial. Dr Roberts never uses it in the acute stage, and always pays attention to the constitutional symptoms.

Erythrophlæa Casca, I have proved an excellent substitute for digitalis; and, in fact, have found it relieve when the latter had failed.

Naphthalin.—This I have found of great service in the treatment of ulcers. Dr Proudfoot was consulted by a patient who had an ulcer on one of his legs, measuring 8 inches by 5 inches. The patient stated that he had had it over fifteen years. His age was 35. He had consulted many surgeons, both private and hospital, but never derived much benefit. Dr Proudfoot blistered the edges, and ordered naphthalin to be dusted on the surface of the ulcer twice daily. At the end of the fourth week of the treatment, the ulcer was entirely healed; and now (two months) there remains nothing but the mark where it was.

Chekan.—As a liquid extract it is excellent in bronchitis and laryngitis.

Tincture of Guarana has become very popular amongst many of my patients who suffer from sick headache. It can be given in doses of 20℥ every fifteen minutes; or, better still, 3j. every hour.

Sanguinaria.—As a tincture, I have found it very useful when the tongue presents a yellow appearance. It seems also beneficial in some forms of jaundice.

Cascara Sagrada.—This drug is now almost too well known to the members of our profession for me to mention. It is doubtless the best remedy for habitual constipation that has ever been discovered. I mention it, however, because I have been obliged to forbid its use to two of my patients, on account of its causing blood to come with the stools. Perhaps others have had the same experience.

Berberis Aquifolium (Barberry).—This has been famed for long as a cure for jaundice. Along with a decoction made from the globe artichoke I have seen excellent results.

Antipyrine.—I have found this new antithermal very useful in acute rheumatism, pneumonia, scarlet fever, and, in fact, in all cases of high temperature. It not only reduces the temperature, but causes a moisture on the surface, followed by pleasant sleep.

Tincture of Jaborandi promotes secretion of the sweat glands in rheumatism and nephritis of scarlatina. It is especially useful in the latter; but I have found that it must not be often repeated, or it will cause greater distress. It lessens the quantity of urine.

Jamaica Dogwood (*Piscidia erythrina*).—This is said to be a specific in facial neuralgia and pertussis. I have found it do good in one case of persistent sciatica. It is an effective anodyne and sedative, with no disagreeable after-effects.

Ethylate of Sodium.—We have tried it in two cases of nævi. The nævi were painted with it once in twenty-four hours. When again seen they were blackened, and had a shrivelled appearance. A second time it was applied, and at the end of three days the blackened crust fell off, leaving little trace of the original mark.

Osmic Acid, or Hyperosmic Acid, or Tetroxide of Osmium.—Used as a 1 per cent. solution, it has proved most useful in cases of obstinate sciatica. In one case in which it was used, the patient had suffered excruciating pain for ten months. He could neither work nor sleep for any length of time. Blistering, morphia, and the battery were tried with only temporary relief. Osmic acid was injected once a day for three consecutive days, then every alternate day for a week, the result being perfect freedom from pain; and now three weeks have passed, and there has been no return.

In laying these notes before the readers of the *Edinburgh Medical Journal*, I do so, hoping that they may prove useful. We have all found that after we have used a whole host of medicines for some painful or troublesome disorder, that our patient still remains uncured, and that we would give almost anything for a cure, to save our reputation. It is for such that I have recorded these notes, hoping that they may prove beneficial both to doctor and patient.

VII.—ANOTHER CASE OF RHEUMATIC PERICARDITIS IN WHICH A MICROBACILLUS WAS FOUND.

By ALBERT WILSON, M.D., Leytonstone, Essex.

SINCE publishing a case last June of acute pericarditis, in which I found a microbacillus, I have had another fatal case of pericarditis in which I have also found a similar microbacillus.

The case and experiments are as follow :—

D. S., æt. 12. The little boy, who was always delicate, came to me in the beginning of September 1885, complaining of pain in the knees, elbows, and shoulders. There was no swelling or tenderness on pressure, but over the knees there were nodules. There were no heart symptoms. During the next three weeks the pains, which I considered rheumatic, disappeared. He became very emaciated, and suffered great pain in the stomach after food. The latter symptom yielded for the time to alkalis.

On the 11th of October I was sent for, as he was worse. I had not seen him myself for three weeks. There was great emaciation, and an expression of extreme suffering. He complained of pain at the heart, and was unable to lie down on account of dyspnoea. At night he slept in a chair leaning forward with his chest on a pillow, a very curious attitude, but the only one in which he could rest and breathe. On examination of the chest, friction was distinctly felt over the region of the heart, and there was a loud harsh double murmur to be heard all over the heart, but loudest over the fourth left costal cartilage. Apex beat normal; pulse, 140; temp. under tongue, 100°; resp., 48°.

Diagnosis.—Pericarditis, with slight serous effusion.

In two days the temp. fell to normal. It rose to 103° on the 16th, and fell slowly and gradually in four days to normal, after 20 gr. doses of sodæ salicylate thrice daily. The symptoms never improved. He gradually got weaker and could retain no food. He complained on the 19th of great pain in the region of the pulmonary artery, and died suddenly on the 21st during a fit of dyspnoea.

Autopsy—Sept. 23rd.—On opening the chest, the heart and pericardium were slightly enlarged. I made an incision in the pericardium, and found both the internal surfaces covered with a layer, an eighth of an inch thick, of organized lymph—very rough on the surface. There were 6 or 8 ounces of pale red serum. On examination of the cavities of the heart they were found to be normal. There was no endocarditis. In the right ventricle there was a large white jelly-like ante-mortem clot, which passed up into the pulmonary artery as far as the bifurcation. Possibly this was the cause of the pain before death. Another ante-mortem clot existed in the right auricle, but this was firmer and more of a fibrinous character. It was of older formation.

As I wished to examine the serum microscopically, I collected some in the following way. I took a small bottle full of eucalyptus oil, and when the pericardium was opened, poured the oil out and filled it with serum, rinsed it, and refilled it twice, keeping the third refilling for examination. For the pericardial experiments, I removed a large piece of the left ventricle, with its rough layer of lymph, and wrapped it in a cloth which was soaked in 1–20 carbolic lotion, and then squeezed almost dry.

Microscopic examination of the serum, an hour after collection, re-

vealed short non-nucleated bacilli, exactly similar to those described and drawn in the June number of the *Edin. Med. Journal*. There were no micrococci. Some of the bacilli moved actively.

Inoculation Experiments.—As the post-mortem was quite unexpected, I was not prepared with any infusions. However, on the 23rd, I prepared in the following way:—

Sterilization of Flasks.—I took twelve 8 oz. wide-mouthed flasks and washed them clean with soap and water. I then filled them brimful with acid sulph. fort. This acid chars all organic matter; therefore, by this simple method the glass was purified inside. I then emptied each flask, and filled it brimful with pure carbolic acid. I then emptied out all except a few drops of carbolic acid, and added some boiling water. I boiled each flask for a few minutes, and then in the carbolic steam spray I emptied each flask, refilled it with rectified spirit, brimful, in order to dissolve all the carbolic acid, poured away the spirit, and holding the flask upside down, ignited the few drops of spirit, and as soon as the flame went out inserted a plug. The plugs were composed of cotton wool, moistened with eucalyptus oil and wrapped in thin muslin, which was soaked in 1-20 carbolic and squeezed almost dry.

Preparation of Medium.—To a sterilized Lister flask I added 4 oz. of Brand's Beef Extract, and filled it up with water which had boiled three or four hours. The whole was boiled for two hours after boiling and filtering. Under the spray I half filled nine flasks. Each flask was kept at boiling point for ten minutes, and then all nine were put in a water bath, which was kept boiling for three hours.

Inoculations.—Under the spray I inoculated three flasks with about $\frac{1}{2}$ a drachm of pericardial serum. Three others I inoculated with strips of pericardial lymph. The remaining three were not inoculated, but kept as a test of the sterilization. The inoculations were done on the 23rd. On the 24th they were put in an incubator at 80° Fahr. On the 26th, the inoculated ones were slightly turbid, and more so on the 27th. The non-inoculated ones were clear. In moving them the plug on a non-inoculated flask came out. It was left out a little time. This flask became turbid three days later and contained bacteria, with slight putrescent odour. The other two flasks kept clear for eight days, when they were used for experiments.

Microscopic Examination of Flasks.—Each flask was examined with a separate clean pipette. All the inoculated flasks contained the same kind of organism, namely, very short bacilli. There was a new feature—some had nuclei, one at each end, which at first sight looked like micrococci in pairs. Furthermore, in one of the flasks there were long threads composed of jointed microbacilli. Many of these threads showed active movement.

I now made a further set of experiments on the action of the two great remedies, quinine and salicin, on these bacilli. I prepared some

fresh infusion of Brand, and sterilized some flasks on the 25th of October. They were 4 oz. flasks, and about 2 oz. of infusion in each. To five flasks I added sodæ salicylate, and to other five soluble sulphate of quinine. I arranged them in varying strengths, namely, $\frac{1}{2}$ per cent., 1, $1\frac{1}{2}$, 2, and 3 per cent. respectively of each. The 3 per cent. solution of quinine was useless, as it would not all dissolve. On the 28th, to each of these flasks I added two things—1. A few drops of the serum inoculated flask, which contained the long threads; 2. A pericardial inoculated flask, which contained the typical bacilli.

After forty-four hours in the incubator the

$\frac{1}{2}$ per cent. sod. sal. flask was turbid.

1 " " " " " " " slightly turbid.

$1\frac{1}{2}$ " " " " " " " " "

2 " " " " " " " " "

3 " " " " " " " almost clear.

There was a slight deposit in each.

$\frac{1}{2}$ per cent. soluble quinine flask was slightly turbid.

1 " " " " " " " " "

$1\frac{1}{2}$ " " " " " " " less turbid.

2 " " " " " " " perfectly clear.

All had a slight deposit except the 2 per cent., which had absolutely none.

Microscopic examination of the salicylate infusions showed fewer organisms as the turbidity decreased. No threads grew in the $1\frac{1}{2}$, 2, or 3 per cent. solutions. The 3 per cent., though almost barren, was not strong enough to destroy the germs. Microscopic examination of the quinine solutions showed poorer growths, while the 2 per cent. solution was absolutely sterile. Therefore, the quinine is the stronger of the two germicides.

On Nov. 1st I prepared two flasks, the one 5 per cent. of sodæ salicylate, the other 10 per cent., and inoculated them as before. They both remained absolutely clear and sterile for a week, at the end of which time I examined them, but could find no organisms. Therefore, while 2 per cent. of quinine prevents the growth of this bacillus, it requires 5 per cent. of salicylate of soda. If this be correct, it will be a guide in treatment. Salicylate of soda acts like a charm in rheumatic fever and endocarditis, but in this disease micrococci have been found, but no bacilli. Rheumatic pericarditis is, so far as my two cases show, associated with bacilli, and quinine is by experience of great value in many bacilli diseases.

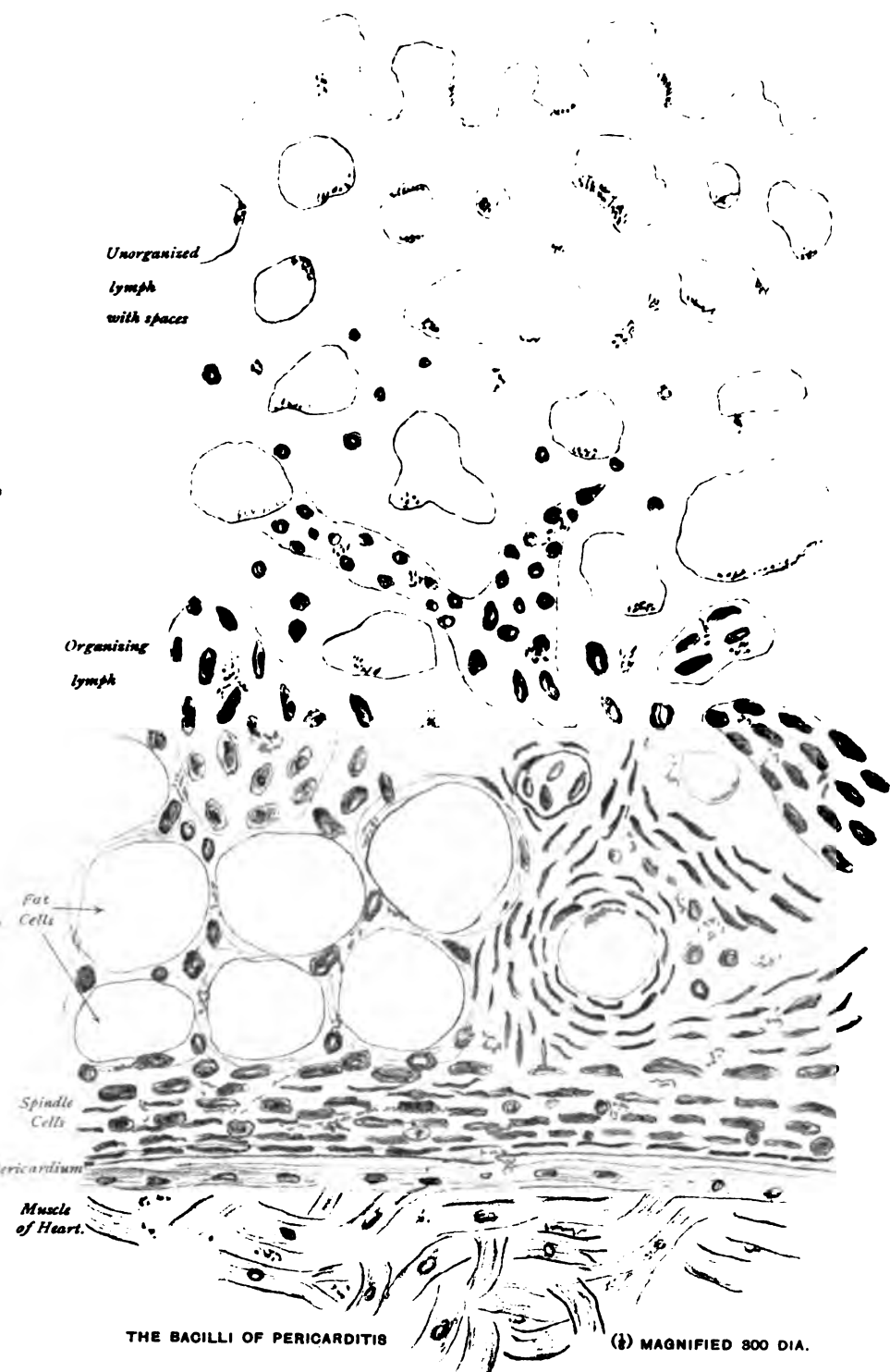
Microscopic Examination.—A portion of the left ventricle, with the attached pericardial lymph, was used for fine sections after hardening in spirit. I stained the bacilli with fuchsin, and used methyl blue as a contrast stain. Since publishing the first case, I stained successfully the pericardium of that particular case with

fuchsin and methyl blue. The bacilli are very numerous, being scattered throughout the effused and organizing lymph, and also penetrating the spaces between the muscular fibres in the muscle of the heart, without apparently causing myocarditis. Probably, if the case had lived longer myocarditis would, as is usual, have ensued, unless a rapid recovery had taken place. The section (see diagram) shows first the superficial lymph, which is structureless, containing fine striæ of fibrine and spaces of varying shape and size; the margin has finger-like processes. The red stained bacilli flourish best in the spaces or vacuoles, being very numerous there, and growing at the margins. A few bacilli also grow on the free edge of the lymph. There are only a few embedded in the lymph at this part. Going deeper, cell formation begins. First, a few single round leucocytes, then clusters, then large round and oval cells, and finally, spindle-shaped cells, evidently the early stage of fibrous tissue. Where fat occurs, the connective tissue surrounding the fat globules contain round cells and numerous bacilli. For at least half the thickness of the muscular wall of the ventricle, bacilli are found lying in the connective tissue. The diagram shows what was seen with a sixth. With the one-tenth immersion they are seen just as represented in my paper of last June—short non-nucleated rods. The latter cannot be made out except with a high power. But the clusters with the sixth resemble what I have drawn.

VIII.—A CASE OF PNEUMONIA OCCURRING AT A HIGH ALTITUDE IN WINTER, TREATED BY VENESECTION.

By A. TUCKER WISE, M.D., Maloja, Upper Engadine.

A. B., an Italian, æt. 50, followed his occupation as a plasterer up to the evening of 8th December. He was seen at 3 P.M. on the 9th. His face was flushed, skin dry, eyes suffused, veins of neck and arms greatly distended; there was a slight irritating cough without expectoration, continuous rigors and violent delirium, calling for restraint. Respirations, 36; temp., 104°; pulse, 108, full, but not of high tension; urine normal. Tongue dry and discoloured in the median line. The chest presented signs of impaired resonance and imperfect entry of air in both infra-scapular regions; vocal vibration was unaltered, but on profound inspiration a few fine crepitations were audible. There was also impaired resonance of both upper fronts of the thorax, with harsh and prolonged inspiration. The heart sounds were obscured by the breathing, and no cardiac impulse could be detected. He was at once bled to about 13 or 14 ounces; and although no immediate impression on the pulse was felt, he became calmer, was thirsty for the first time, and slept for two or three hours.



10th December.—Had passed a good night with the exception of wandering delirium for half an hour. The pulse at 9 A.M., 92; temp., $102^{\circ}2$. The distended state of the veins had quite disappeared; the heart sounds were audible, and apex beat felt in normal position; the urine was loaded with lithates, and pain over the kidneys lessened; conjunctivæ less red and injected; tongue moist. He was rational and complained of much thirst; there was now rusty-coloured gluey expectoration. Over the right infra-spinous and infra-scapular region there was absolute dulness, increased vocal resonance with tubular breathing. On the left side, slight dulness over ill-defined patches of the lung. Air entered less laboriously in front of the chest; the aspect and manner of the patient was greatly changed for the better. Warm red wine was given during the night, milk by day, and ice for the extreme thirst. Although the room this patient occupied was inhabited by 28 other workmen, and was used for cooking, eating, sleeping, etc., no windows were ever opened. An iron stove served for heating purposes; but in spite of such conditions the temperature was so low that the wall by the side of the man's bed was covered by a layer of ice, about an inch in thickness. Roughly estimated, the cubic capacity was 400 cubic feet for each man.

11th December.—Temp., $100^{\circ}5$; pulse, 96; respirations, 34.

12th December.—Temp., $100^{\circ}4$; pulse, 92; respirations, 32. High-pitched friction sound under right nipple, limited to an area of one square inch. Dulness over right side remains; clearing on left. Skin moist.

13th December.—Temp., $100^{\circ}5$; pulse, 84; respirations, 34. Spirits depressed by pain in side. Mustard leaf applied.

14th December.—Temp., $100^{\circ}5$; pulse, 80; respirations, 32. Slept well after a morphia injection; cough diminishing; sputum less bloody. Air enters feebly in both backs. Vocal resonance still slightly increased, mostly on right side, with harsh breathing, bronchial in character. Friction sound extended in area, but softer and painless. Five grains of quinine were given as a powder at 10 A.M. Free perspiration occurred during the afternoon.

15th December.—Temp., $98^{\circ}9$; pulse, 68, of fair volume and force; respirations, 24. Slept well during the night and coughed less. Sputum still tinged a little with blood. A crop of herpes had appeared on the lips, and there was a general loss of flesh.

17th December.—Temp., $98^{\circ}4$; pulse, 60; respirations, 26. Dulness over the lungs was lessening, sputum uncoloured, cough diminished.

23rd December.—He went out for a walk in the sun, wearing a cardigan jacket but no coat. The temperature of the air at this time was 24° Fahr. As Christmas was near, he insisted on leaving the Maloja for his home on the borders of the Lake of Como, and quitted the Engadine on a sleigh the next morning; no

harm came of it, and he resumed work about two weeks afterwards.

Remarks.—The result of blood-letting in this case (which, with the exception of 5 grains of quinine, a mustard leaf, and a few hypodermic injections of morphia, was the only active treatment adopted) appears to have been in all respects satisfactory. Had recourse been had to other than direct means for relieving the engorgement of the venous system, much valuable time must have been lost; but by depletion, the pressure that might have resulted in profuse exudation into the air cells of the lungs was relieved mechanically. The disappearance of the delirium and production of sleep demonstrated the benefit obtained by the venesection, whilst the temporary effect thus gained was followed by rapid changes towards convalescence. The apprehension of weakening a patient in such a condition, by removing venous blood (which even if it could pass the lungs would carry but little oxygen with it) appears quite groundless.

Although venesection is scarcely ever practised at these altitudes, it is clearly indicated in sthenic pneumonia, especially if we accept the theory that the lungs contain a larger proportion of blood at cold altitudes than at lower and warmer levels. The increased pulmonary vascularity in natives and persons visiting the Swiss mountains has never been absolutely proved; but observers have shown that the lungs are heavier in cold climates than in the tropics; secondly, these organs are brought into greater physiological activity in mountainous districts, and should on this account have a larger blood supply. It is a curious fact that whilst pneumonia is not uncommon amongst the natives of the high valleys of Switzerland during spring time, it may be said never to run into phthisis. Can this be due, in part, to the lungs containing more blood than at lower latitudes? We know that in mitral disease, where the lungs are loaded with blood, phthisis is very rare.

IX.—INTRODUCTORY ADDRESS TO A COURSE OF LECTURES ON DISEASES OF THE TROPICS AND CLIMATOLOGY.

By ROBERT W. FELKIN, M.D. et Chir. (Marburg), F.R.S.E., L.R.C.P., L.R.C.S. Ed., F.R.G.S., Member of the Anthropological Societies of London and Berlin; Member of the Scottish and Berlin Geographical Societies; and of the Obstetrical Society of Edinburgh, etc.

(Continued from page 852.)

I NOW come to my second point, Malaria, which constitutes so fatal a hindrance to acclimatization in the tropics, for malarial diseases reach their maximum of frequency and intensity in tropical and sub-tropical regions. It is not always easy to settle exactly where malaria ends or the hot climate begins to exert its influence,

for in most tropical countries which are as yet known to us both go together, and it is difficult to find places where malaria does not exert more or less influence.

The literature on the subject is immense. In Hirsch's book of *Geographical and Historical Pathology* no less than 16 pages are taken up by the enumeration of the works on malaria by writers since the 16th century.

The etiology of malaria has always been a disputed point, and to the present day many theories respecting it are advanced and strongly advocated. One theory, however, appears to gain ground, that whatever may cause malaria, there is only one malarial poison; and that, though it has a very wide geographical distribution, we may assert that the mean summer isotherm of 60° F. limits the production of it, and that those regions where the mean summer temperature does not reach that height are exempt from this disease. A high temperature alone, however, will not produce malaria. Moisture of the atmosphere, the influence of winds, the altitude and configuration of the ground, the geological and physical characters of the soil, its saturation and the amount of organic matter it contains, as well as cultivation or non-cultivation, all exert a powerful influence in the production of the malarial agent, whatever it may be. A few very remarkable facts show exceptions to the rule that marshy soil is malarious. For instance, Humboldt pointed out that the marshy banks of the Amazon in the upper part of its course are free from malaria, and this has been confirmed by Bates. Mantagazza says, with reference to the banks of the Río de la Plata, "Paludal fevers are nowhere known;" and Dupont remarks "Intermittent fevers are entirely unknown in the littoral of the la Plata. It is difficult to say to what cause their absence is to be attributed, but all physicians are agreed that they are absent." The immunity from malaria which New Caledonia enjoys is also very striking, in spite of a mean annual temperature of 73° F., and the fact that, as Charlopin says, "all the conditions favourable to the existence of malaria exist at various parts of the island, and especially at the parts most inhabited."

On the other hand, malaria is sometimes found in very dry places where no one would expect it. Marked examples of this occur on the tablelands of the Deccan, on the slopes of the Rocky Mountains, and in the bald, arid, sterile tablelands of New Castile. Lastly, malaria, there can be no doubt, can be carried by winds and water. To what then is malaria due?

Griesinger says, "Without the assumption of a material and specific malaria poison, we shall not proceed far in explaining the endemicity of the fever." This statement was made to confute the belief that malaria was due to the influence of weather alone, in fact, to a chill, as Dr Oldham maintained. I do not intend to mention all the theories that have at one time or other been advanced as to the cause of malaria. Suffice it to say, that it has been attributed to the toxic excretions of living organisms, plant

or animal; to an unknown gas emitted by marsh water. Some say it is due to a parasite, others to some telluric influence. Dr Munro ascribes it to certain electrical conditions, and Klebs and Tommasi-Crudelli claimed to have proved it to be due to a germ which they discovered and cultivated, and from the cultivation of which they produced malaria in the lower animals. Ceci and Marchand appear to agree with this theory, as does also Marchiafava, who found the same bacillus malarie in the bodies of several persons who died in Rome of pernicious malarial fever, and Griffini has confirmed the occurrence of them in the Lombard rice fields. I am myself very much inclined to agree with this view, for I have examined germs taken from my own blood when suffering from malarial fever, and find that they coincide remarkably with those depicted by Klebs and Tommasi-Crudelli. In this connexion I would like just to notice a circumstance which I do not think has been mentioned by any other observer. It is that the germs appear to cluster in a remarkable manner around the white blood corpuscles. Perhaps this may be found to explain the action which quinine exerts upon malarial poison. I show you a diagram made from a drawing of my own blood in which you will see this peculiarity. Into the details of this subject I cannot enter now, but reserve them for a subsequent lecture. To whatever cause malaria may be due, no race enjoys perfect immunity from its attacks. The negroes are perhaps the least predisposed to it, but the partial protection they possess has been acquired, and refers only to the district and the conditions in which they continuously dwell. I have witnessed myself on many occasions how much they suffer when removed from their homes to a new malarial district. They are also attacked as much as other races in districts which are brought under cultivation for the first time. I may here just refer to an instance of the severity with which malaria will attack negroes. It happened when I was at Mruli in Central Africa. The excavation of a new moat was commenced early one morning, and of the 37 negro soldiers employed, 29 were struck down by fever before 10 A.M., and of these 17 were buried before sunset, notwithstanding the strenuous efforts I made to combat the malady. The mortality in constructing the Panama Canal further illustrates this point.

That the cultivation of the soil makes a place healthier is true to a certain extent; but, on the other hand, in many tropical districts a previously healthy place has been rendered highly malarious by the introduction of the cultivation of such plants as cotton, sugar-cane, or rice, on account of the irrigation necessary for their production. An instance of this kind occurred some years ago in Mauritius, which was previously free from malaria. The cultivation of the sugar-cane was introduced, and a most pernicious type of malarial fever resulted, which remains endemic to the present day. Lower Egypt, too, was once practically free from malaria, but the introduction of cotton plantations, and further up the Nile

the cultivation of the sugar-cane, were followed by the production of malarial fever, to which the name of Dinka fever has been given, on account of the remarkable susceptibility of the negro labourers to its ravages. In the seventeenth annual report of the Sanitary Court of the North-Western Provinces and Oudh, it is stated by Dr Plane that "the mean fever ratio of the twelve districts of canal irrigation was, in 1884, 33·3, in comparison with the provincial fever ratio of 24·3." These instances will show that although the natives of a district, uninfluenced by foreign customs and leading their ordinary lives, may be very little inconvenienced by malaria, no sooner do they move to a new district, or cultivate the ground more extensively or by new methods, than they may be attacked by fever to almost as fatal a degree as are the white races.

It is said by some that the skin of Africans possesses a more highly organized excretory power than that of Europeans, and that they are hence provided with the means of getting rid of the morbid agent. Fritsch holds this view, and there is something to be said for it. Probably in instances where negroes suffer from the pernicious forms of fever, it is due to their exposure to an unusually large amount of malarial poison, with which even their constitution is incapable of dealing.

We have no reason to suppose that any individual or race can become entirely proof against malarial poison, and I cannot believe that Europeans will ever be exempt until we know more of the cause of malaria, its prevention and cure. I do not mean by this to imply that some methods of cultivation, drainage, and other such sanitary precautions may not improve a malarious district—far from it; but in considering the subject of acclimatization we must remember that all such improvements cannot be effected in a day, and therefore the colonization of any malarious district will be invariably attended by much sickness and probably serious loss of life.

We must not forget that every new malarial attack strengthens the predisposition to another, and pathological investigation shows that every new attack leaves behind certain changes in the spleen and other organs, and therefore a complete restoration to health of the individual who has once suffered from malaria is a matter of great difficulty; while subsequent diseases may be complicated in their character and course by a previous residence in a malarious district. It is important and curious to note that a person may visit a malarious spot for a short time with apparent immunity, and yet later experience show that he has not escaped, for after many years lying dormant the germ or poison within him may be excited into action. A case occurs to my mind in which a gentleman suffered from a severe attack of ague, and yet he had only been exposed to malarial influence fifteen years previously, at which time he had been apparently unaffected by it.

Monsieur Bertillon has somewhat succinctly summed up in four classes the circumstances which go far to prevent sudden

acclimatization in new isothermal regions. They are as follows:—
1. Acute diseases, many of them endemic. 2. Chronic consecutive anæmias, which place the individual in an unfavourable position to resist accidental disease. 3. Disease in early infancy in offspring born in the new home. 4. Physical and intellectual degeneration and the infertility of the second and third generations.

It seems to be a fact that in nearly all foreign places the children of the first, but more certainly of the second generation of colonists die off early, and this is not only the case in districts affected by malaria, but also in others. Look at Egypt, for instance. True, it is in the tropical zone, but so salubrious that Europeans go there in search of health, and, except in the places I have previously mentioned, it is free from malaria, yet no children of northern European families thrive there. The reason for this lies very deep, and still further investigation will be necessary before we can satisfactorily answer the question, "Why do the children die?" In tropical countries I hold that the race dies out, not so much because of the infertility of the women, but because of their inability to rear the children. But in many cases we shall find that where the woman is sterile it is due to the action of the climate upon her as a European, and not to any specific action on her because of her sex. Many authorities are of opinion that women suffer far less from fever itself than do men, and this statistics seem to prove. It is perhaps because they lead lives much less laborious than the men; and I cannot avoid thinking that it is the luxurious, idle, and frivolous lives which European women so often lead in the tropics that unfits them to fulfil maternal functions.

While it is undoubtedly true that there are many difficulties in the way of acclimatization, I believe that medicine and hygiene have already done much to remove the great barriers to its accomplishment, and I hope that still more will be done in the immediate future. If we examine the death-rate of Europeans in India and on the west coast of Africa, we see what a change for the better has taken place during the last 50 years. People now know much better how to guard against the preponderating influence of external agencies, and can more easily procure a change of climate when it is necessary for the re-establishment of health. On the other hand, one of the greatest dangers to Europeans now going to the tropics is the great rapidity of the change of climate, and in this respect steam navigation is not an unmixed blessing. It carries troops or passengers so speedily to their new home, that they have no chance of getting accustomed to the heat of the tropics.

I willingly allow that much has been done in the past for the health and well-being of Europeans in the tropics, but we must make still further progress in the study of climate and climatic disease before the time shall come when the death-rate in these far lands will not exceed our own. Before this wished-for time arrives many observations must be made, much patient work be done, and many experiments performed. To whom are we to look

for the accomplishment of these investigations if not to the medical men who go out from us, and who in their practice have constant opportunities presented to them for making unlimited research and useful discovery?

This leads me to remark that the subject of meteorology should form a part of your studies. In a subsequent lecture I shall show you the instruments that are used, and how to observe with them. Also how to draw up reports on climate, and to make medico-topographical charts of the districts in which you reside. I expect that you will soon grow interested in this study, and, as it is by no means difficult to obtain proficiency in recording the observations, I am sure that you will not regret the short time I shall be able to devote to it. I feel very strongly that every medical man should make systematic observations when abroad. Most of the instruments will be gladly given you by one or other of the meteorological societies, if you only promise to send them a copy of the results you obtain; and as every set of observations is of great value, it is well worth the slight trouble they entail.

Meteorological stations must be established all over the globe before the true value of climatic influence can be really estimated, as events take place in far off lands which exert much influence on our own climate. We laugh sometimes at the American predictions of storms, but we laugh from ignorance, for already the good that these warnings do for our sailors is inestimable. The more perfect the chain of meteorological stations, so much the more accurate will the scientific weather forecasting become.

The importance of the facts I have hinted at is very great. Year after year new fields for commerce and for emigration are being eagerly sought out, and mankind will owe a new debt of gratitude to our profession, when we can control or conquer influences which so fatally resist our advance. This victory will not be won until the influence of climate on the physiological actions of the body and upon disease are better understood than at present, and the field of work opening before you is not only of importance but of vital interest to science, to the progress of our empire, and the spread of our religion in all quarters of the globe.

Gentlemen, in carrying on practice in foreign lands, it is necessary to consider the condition of the people among whom you reside. You must remember that they are very different from ourselves, and you cannot expect them to appreciate at once or intuitively the advantages of the sanitary laws and precautionary measures which it will be wise to inculcate. Bear this in mind in all your dealings with foreign races. It is important also to pay great regard to the influence of religious or superstitious beliefs which obtain among the races with which you will come into contact. They exert a great and powerful effect, not only on the nature and severity of disease, but also on the means available for its treatment, more so than may at first sight appear.

It will well repay those of you who intend to practise abroad to

give some attention to the ideas of gods and demons, and of spirits of departed friends, which to such a great extent impress the native mind, and to weigh many circumstances which you might now think to be outside the pale of your professional studies. Do not think that ideas which to us are strange are to be unheeded or scorned. We must remember that we ourselves have superstitions, which even yet exert powerful action for good or for evil upon our minds. At no very distant date the inhabitants of this land were in much the same condition as the African of to-day, who, in Uganda, for instance, worships the god of smallpox and offers gifts to appease his ravages. To the natives the air is full of spirit forms, who influence their lives and affect their health. Can we wonder that sleep, dreams, death, should have a powerful fascination for them? and when they tell you that they believe that the soul leaves the body during sleep, or that departed spirits have visited them in their dreams, or that disease and accident are caused by some vengeful sprite, you must not deride them. Can you wonder that epilepsy is thought to be due to evil spirits, as it is in Darfur? or that mania is considered a demoniacal possession, as it is among many races? We should always respect the beliefs of others, even if held by savages and seeming ever so frivolous. Teach them better, higher things if you can, but beware how you break down native customs and laws till you can give them something superior, more stable in exchange. I remember once in Africa being placed in an awkward position through ignorance of native beliefs. I was accused of murder, nay more, I was tried for it, and why? One night I was much annoyed by the howling of hyenas, and my anger being aroused, I shot one. Next day a fearful commotion took place in the native village, and one of the men brought a solemn charge against me of murdering his grandmother, for there they believe in the transmigration of souls. My conscience acquitted me, not so the judge, and a heavy fine caused me to repent my folly.

In some places before you venture on an operation, or even on giving medicine to a patient, you may find it is well to use great caution. This is especially to be borne in mind by any who may go as missionaries to very uncivilized tribes. Make a contract before you operate, and insist that you will not be answerable for the result. It is well also sometimes to explain what will happen during an operation. I well remember the first operation I ventured to perform in the Soudan. At Berber, an Arab came to me with some half dozen friends, and asked me to remove a large unsightly tumour from his head. I consented, but no sooner had I made my incision than my dragoman cried out, "Allah kerim (God be merciful), he has cut into his brain." In a moment daggers were drawn and a scene of the utmost confusion occurred. This would probably have been my last operation had not my kavasses had more faith in me than my dragoman, and sprung between me and my patient's friends.

In dealing with even simple disorders a wise physician must take into account the manners, customs, and individual or tribal idiosyncrasies of his patient, mental as well as physical, if he would have his medicines act like a charm. He must remember that religion, as apprehended by different nations, often affects the very possibility of applying the more exact knowledge that western investigation has rendered possible and real. These religions or superstitions, however, are a part of the nature with which you have to deal. They react upon the physical constitution, and must be taken into account, not as matters of prejudice, as we British are only too apt to do, but as a part of that heredity whose influence is now one of the great acknowledged facts of medical science. A man, who cares nothing or knows nothing of Arab blood feuds, of negro superstitions, or of Indian caste, will soon find himself at a disadvantage, even if he does not get involved in some great difficulty. He may have been the best man of his year when he passed his final, but unless he takes some means of making himself acquainted with the climate, diseases, and the inhabitants of the land in which he intends to practise, he will grievously fail in his loyalty to his profession, to his patients, and to himself.

It is the duty which a medical man owes to science, to observe with critical, but at the same time with no unfriendly or sarcastic eye, the acquired skill and the empirical remedies used by uncivilized races; as, for instance, the use of unguents, of baths, massage, of waters, and numerous other remedies; for their employment indicates, if we examine them carefully, that there is either some real physical need or great desire which they satisfy, or perhaps tendency to disease against which they may act as prophylactics.

A point on which you must be warned, and which at the same time opens up a wide field for observation and research, is the influence of climate and race in modifying or causing unusual susceptibility to the action of drugs. Doses of certain medicines which you could give in Europe with perfect safety might kill at the equator, and doses which Africans would enjoy would prove injurious if given to Europeans in Africa. In travelling up to the sources of the Nile I noticed what I may call three zones of susceptibility to the action of opium and quinine. Doses from which one obtained good results near Khartoum induced unpleasant if not dangerous symptoms in Uganda, and one or two drugs which proved injurious to the Arabs were of much service to me in treating Niam-Niam cannibals. And the intolerance which the Dinkas evinced to tartar emetic makes me regret to the present day some half grain doses I administered.

In surgery, too, you will find a wide diversity in the possibilities and methods of treatment. Operations which you would rarely perform here you can carry out with safety in Africa, such is the marvellous strength of the *vis vitæ* in some uncivilized races.

Gentlemen, I have read of an image of *Æsculapius* which represents him, not as an aged and crafty physician, but as a youth, earnest and strong of aspect, carrying an ampulla in one hand and in the other a traveller's staff. This pilgrim guise aptly illustrates to us how one chief source of the Master's knowledge of healing had been derived from observation of remedies resorted to by animals labouring under disease or pain; what leaf or berry the lizard or dormouse laid upon its fellow. It was for this that he for years had led the life of a wanderer in wild places.

May a like spirit of keen inquiry and patient investigation inspire you in your studies, and ennoble you in your future practice.

X.—PULMONARY PHTHISIS IN CONNEXION WITH OCCUPATION AND WITH OTHER DISEASES.

By ALEXANDER JAMES, M.D., Assistant Physician, Royal Infirmary, Edinburgh.

As has been stated previously, the mortality from phthisis may be regarded as one of the sacrifices which nature demands from humanity in return for her good efforts to transform the struggle for existence from the physical to the intellectual, that is to say, in return for the advantages of a progressive civilisation. This is indeed obvious. In savage or nomad life the physical is the element in man which is mainly developed, and although, in such circumstances, struggles have to be engaged in and sacrifices are entailed, he enjoys there an amount of breathing space, a freedom of movement, and an exposure to sun and air, which, by promoting healthy lung function in a healthy surrounding, renders chronic disease of that organ little liable to occur.

Civilisation, however, inasmuch as it is only possible when numbers of individuals are brought together, alters all this, for all statistics and observations demonstrate that where the population is most dense, and where, therefore, individual movement is most limited, and air least pure, there phthisis is most prevalent. Naturally such a statement as this, as regards phthisis, is only a general one, and is subject to many qualifications, but its truth is distinctly evidenced on comparing the amount of this disease in urban and rural districts, and in counties in which the inhabitants are nomadic, pastoral or agricultural, and industrial.

Among industries, some are, of course, more hurtful than others, and the evil effects are brought about in a variety of ways. Thus deficient physical work is one of these, as shown by the number of engravers, watchmakers, compositors, clerks, tailors, etc., who become phthisical, and although in such industries the impure heated air with which they are usually associated must aid in the development of the phthisical process, yet the lessened respira-

tory function which such occupations entail must have a very important effect. Thus Dr Guy¹ showed that in the London printing establishments, of compositors and pressmen, the former were more prone to consumption, and he attributed it to the fact that although both worked in the same heated impure atmosphere, the former had little physical work, whilst the latter had every now and then to perform work requiring a great amount of physical exertion.

As stated above, deficient physical work and impure air usually go together, but as evidencing the effects of the latter, the comparative mortality from phthisis among fishermen, agriculturists, grocers, and drapers may be compared. Dr Ogle, in his Supplement,² gives their relative numbers as follows:—Fishermen, 108; agriculturists, 115; grocers, 167; drapers, 301. Breathing an atmosphere in which dust particles are suspended is well known to be an important factor in the causation of lung disease, and specially of phthisis, and the amount of noxiousness seems to depend on the quality, as well as, or probably more than, on the quantity of the dust inhaled. Thus the dust of coal, wood, or flour seems to be less injurious than that given off by textile fabrics, as in cotton or wool factories, whilst these latter are exceeded, as regards injurious effects, by the metallic and mineral dust. In evidence of this Dr Ogle gives the following as the relative mortality from consumption and respiratory diseases among individuals whose occupations entail dust inhalation:—1. Coal-miners; 2. Carpenters and joiners; 3. Bakers and confectioners; 4. Masons, builders, bricklayers; 5. Wool manufacturers; 6. Cotton manufacturers; 7. Quarrymen; 8. Cutlers; 9. File makers; 10. Earthenware manufacturers; 11. Cornish miners. That particles of metal or stone, when inhaled, should prove more irritating than those of flour or wood can be readily understood, but there is no doubt that (as Dr Ogle has pointed out), in drawing conclusions as to the relative injuriousness of the different kinds of dust from a table such as this, great care must be exercised. For example, the mortality from consumption among coal-miners is on the whole so low, and compares so favourably with that of many occupations in which there is no dust inhalation, that in the opinion of many observers coal dust is not only not a provocative of consumption, but, indeed, acts as a preservative from this form of disease.³ Again, the liability of bakers to consumption has certainly other causes than flour dust inhalation. The heated and moist atmosphere in which they work, and the sudden changes of temperature to which they

¹ "Influence of Employments on Health and Disease," *Statistical Society's Journal*, vol. vi. (1843), p. 204. Since then the introduction of machinery has nullified this distinction.

² Supplement to the 45th Annual Report of the Registrar-General.

³ Walshe, *Diseases of the Lungs*, 4th edit., p. 226. Compare also Niemeyer, *Practice of Medicine*, p. 197.

are exposed, probably act in this manner much more potently, and this is borne out by the fact that millers who, as far as dust is concerned, breathe a similar atmosphere, are, as a rule, comparatively free from phthisis, and healthy.

Further, the liability to this disease from which masons, carpenters, joiners, etc., suffer must also be looked upon as being not altogether due to dust. Their work, carried on in sheds or in houses not yet provided with doors, windows, etc., entails constant exposure to cold and draughts. For the last few years I have made a habit of asking such, whenever they come to me as patients, to what they attribute their illness, and I have observed that, although they all understand the injury which dust is apt to produce, they almost invariably ascribe to the cold draughts, etc., the greater share in the production of their complaint. What share each of these different morbid agencies has it is, of course, impossible to form any very definite idea; but with a view to elicit some information on this point, I have drawn up a table (Table I.) to represent the relative liability of individuals of different occupations to consumption and certain other diseases. The occupations which I have selected are coal-miners, printers, carpenters, farmers, masons, tailors, bakers, butchers, and cab and omnibus drivers, these being chosen as representing occupations some of which are regarded as affording a special immunity from the disease, as farmers and farm labourers; others a special liability to it, as masons, printers, etc.; and a third, the effects of which on it are somewhat doubtful, as butchers, cabdrivers.

This Table has been drawn up from the data given in Dr Ogle's report (Table L. p. 32).

TABLE I.—Comparative Mortality of Males, 25 to 65 years of age, in (selected) different industries from (selected) different causes.

	Diseases of the Nervous System.	Diseases of the Circula- tory System.	Phthisis.	Diseases of the Respira- tory System.	Diseases of the Urinary System.	Liver Diseases.	Alco- holism.	Gout.
Farmer, Grazier..	67	70	46	55	75	105	60	66
Cab - Omnibus- Service.....	112	133	163	187	158	138	330	366
Butcher.....	116	110	118	114	134	246	230	166
Baker.....	114	109	96	102	97	117	150	166
Tailor.....	121	105	129	102	109	123	110	133
Printer.....	75	77	209	91	73	70	30	...
Mason, etc.....	73	95	114	110	119	76	50	100
Coal-miner.....	77	77	75	78	86	91	30	...
Carpenter, etc....	74	86	92	73	95	92	40	66

TABLE II.—Mortality from Phthisis relative to Mortality from other Causes mentioned in above Table (Gout excepted).

Farmer, Grazier, . . .	1 9.39	Tailor, . . .	1 5.19
Cab- Omnibus-Service, .	1 6.49	Printer, . . .	1 1.99
Butcher, . . .	1 8.05	Mason, etc., . . .	1 4.58
Baker, . . .	1 7.17	Carpenter, etc., . . .	1 5.00

Looking only at the amount of phthisis in men following these occupations, the list in order of liability runs thus—printers, cab and omnibus drivers, tailors, butchers, masons, bakers, carpenters, coal-miners, farmers and farm labourers. This gives a high phthisis mortality to cab and omnibus drivers and butchers, and a correspondingly low one to carpenters, joiners, and coal-miners. If now, however, we estimate the phthisis mortality in proportion to the mortality from the other causes mentioned in the table (excluding gout, because no cases of it occur among printers and coal-miners), a somewhat different arrangement is obtained. In order of frequency they now are—printers, masons, carpenters, tailors, coal-miners, cab and omnibus drivers, bakers, butchers, farmers, and farm labourers.

This, except that it appears to give a too favourable aspect to the bakers, much more nearly corresponds with hospital statistics than does the former arrangement, and it is interesting as giving to cab and omnibus drivers and butchers a comparative immunity. As is well known, this opinion as regards butchers has long been held. It is combated very strongly by Dr Ogle in his report; but in favour of it it might be held that in their case, as in that of the cab and omnibus drivers, the liability to phthisis is accidental, and due mainly to alcohol, and that it is relatively not nearly so great as to other diseases, as of the kidney, liver, and nervous system.

This leads to a very important question, viz., that of the existence of certain diseased conditions on the one hand predisposing, and on the other antagonistic to phthisis. On this point much has been written, but the opinions expressed are very diverse. As examples of diseases considered in this connexion the following may be quoted: Cancer, rheumatism and gout, ague, lunacy, diabetes, typhus and typhoid fever, heart disease, emphysema, etc. From such a list it is evident that a classification into two main divisions may be made—the first comprising these diseases, the effects of which on phthisis must be due to some influence which they exert on the organism as a whole; the second comprising those which act by modifying in one way or other the respiratory function. The first may be represented by cancer, insanity, or ague; the second, by heart disease or emphysema.

It must, however, be remarked that the dividing line is not a hard and fast one. In ague, for example, the general system is profoundly affected, but the big spleen may directly affect lung function; and again, although in a case of heart disease this lung function is directly influenced, the constitutional condition which has led to the development of the cardiac mischief has necessarily

also to be considered. The classification, however, has its advantages, and so may to a certain extent be followed.

Cancer has, by many observers, been regarded as antagonistic to phthisis. It was so, among others, by Rokitansky; and such observers not only considered that the diathetic conditions leading to the development of these two diseases were distinct, but as indicating this distinction more decisively, they instanced their tendency to affect different tissues. Thus Rokitansky¹ gives the following list:—

<i>Frequent.</i>	<i>Rare.</i>
Lung tubercle.	Lung cancer.
Ovarian cancer.	Ovarian tubercle.
Salivary gland cancer.	Salivary gland tubercle.
Stomach cancer.	Stomach tubercle.
Œsophagus cancer.	Œsophagus tubercle.
Ileum tubercle.	Ileum cancer.

He also refers to the fact that whilst tubercle tends to affect the apex of the lung, cancer affects every part of its tissue, and that similar differences as regards precise locality are to be found in connexion with the uterus and testis. By subsequent writers this idea of an antagonism has been disputed. That they coincide so seldom in the same individual is explained away as resulting from their tendency to affect the organism at different ages, cancer in this way being no more antagonistic to phthisis than is apoplexy, whilst as against there being anything like a distinction in the diathetic conditions, it has been shown that carefully investigated cases of cancer show a strong phthisical tendency in the family history, and *vice versa*.²

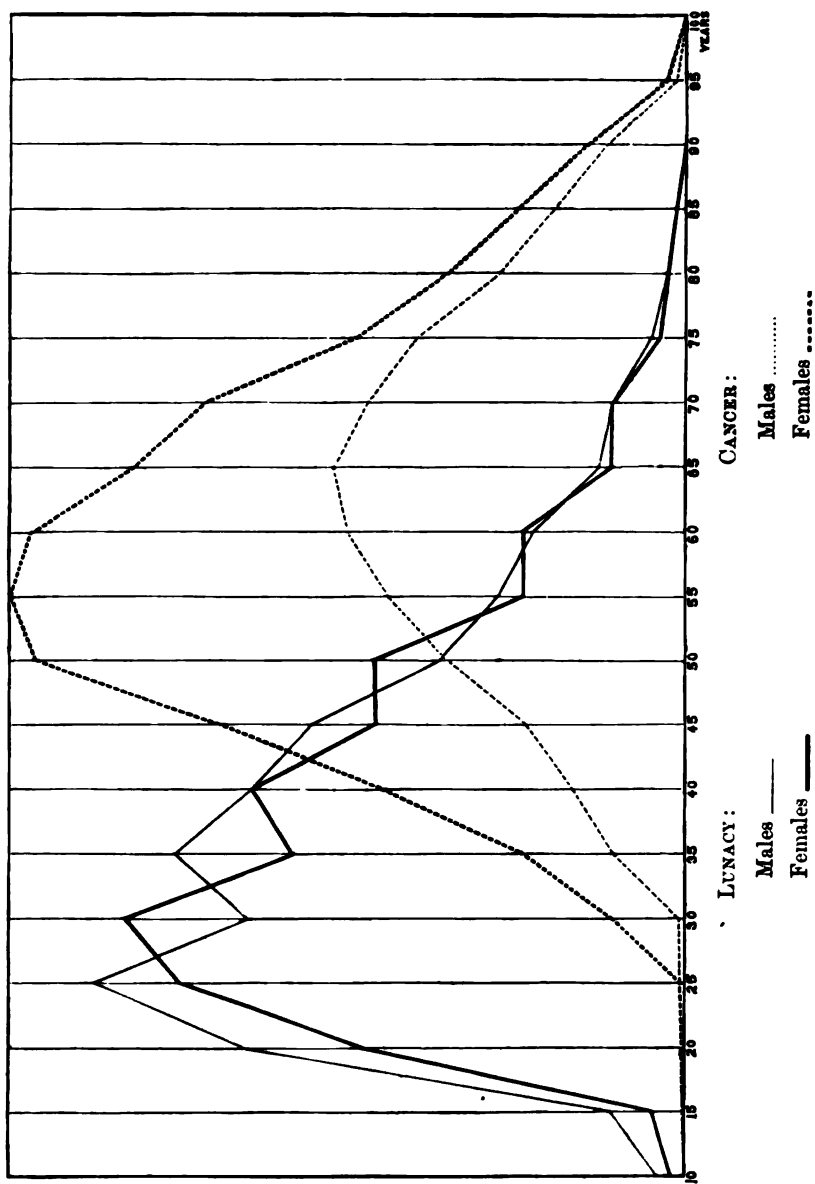
But these diseases may be compared in other aspects. As regards sex, for example, an agreement is found, both occurring most commonly in females. This is shown by the curve, which represents the proportionate male and female mortality from cancer in Scotland during the ten years 1870 to 1879. A further point is that the maximum is reached earlier in women (55 years) than in men (65 years).

But on comparing the relative mortality of the sexes from this disease at different ages, a remarkable difference is found, for the proportion of women who die of cancer in early years is *less*, and in old age *more* than that of men,—a condition of matters the reverse of that which, as we have seen, exists in phthisis.

The fact that the cancer mortality, like that of phthisis, reaches its maximum earlier in women, may be explained by her period of growth, as distinguished from maturity and development, being shorter than that of man, but that up to the 25th year, or thereabouts, female cancer should be less common than male, whilst female phthisis is, as we have seen, more common, is peculiar.

¹ *Pathological Anatomy*, vol. i. p. 373.

² Compare Weeden Cook, *Med. Times*, 1867, i. p. 538; also Cahen, *Centralblatt für Klinische Medicin*, 1885, p. 860.



In conjunction with the fact that women are longer lived than men, and that in old age female cancer is commoner than male, it would seem to denote that although these diseases agree in indicating a defective constitution, they are yet essentially different; that whilst phthisis is the result of the effects on the organism of unfavourable surroundings, cancer is to be regarded more as a sign of degeneration or senility, abnormal, perhaps, mainly because occurring at too early an age.

Insanity.—The close association of phthisis with insanity as an item in family history, and as a disease which is specially prevalent in asylums, has been acknowledged,¹ and the importance of trusting to physical signs rather than symptoms in connexion with the diagnosis in the latter class of cases has often been adverted on.

Phthisis and lunacy seem to agree in both being most frequent in females, and the form of insanity which as regards age most closely corresponds with phthisis is mania.²

When next we compare the relative ages at which insanity affects the sexes, we find, however, differences. Thus the curve³ shows that in the early years it is most frequent in males, reaching its maximum at 25; less frequent in females, reaching its maximum at 30; and that in later life the liability is almost equal at the different ages. Here, then, we have with insanity, as with cancer, a condition different from that which obtains with phthisis. That both of these show in early life a greater tendency to affect males stands out strikingly. It is to be noted, however, that insanity differs from cancer in having its maximum later in females.

The association of phthisis or the phthisical tendency with insanity in the same individual next deserves attention. A phthisical insanity, *i.e.*, an insanity occurring in individuals in whom there is a strong tendency to phthisis, is recognised. It is of the "mildly delusional, slightly demented type,"⁴ and moodiness, suspicion, and melancholy seem to be its most usual characteristics; and the importance of these mental conditions, as associated with phthisical insanity, is strengthened by the observation⁵ that they are likely to be induced when in a case of previously existing insanity phthisis supervenes. Riva and Suphide⁶ also state that phthisis is more common in cases of depression and of mental decay than in the insanity of an exalted kind, and they point out also that no "metastasis" of any kind occurs, *i.e.*, that

¹ Walshe, *ibid.*, p. 455.

² As distinguished from melancholia and general paralysis. Clouston, *Mental Diseases*, p. 206, shows mania to reach its maximum between the 25th and 35th year.

³ This curve is constructed from 1785 male and 2200 female cases admitted into Morningside Asylum. For these data I desire to express my thanks to Dr Clouston.

⁴ Clouston, *ibid.*, p. 461.

⁵ Clouston, *ibid.*, p. 472.

⁶ *Physical Diseases in the Insane*, Riva and Suphide. See reference in *Lond. Med. Record*, 1879, p. 479.

where phthisis supervenes on insanity, or insanity on phthisis, nothing like an amelioration in the symptoms of the primary affection can be detected.

It will be at once remarked that such a mental condition is very different from that with which medical men are for the most part familiar in ordinary cases of phthisis, where the mental faculties, if affected at all, are as likely to evince an increased acuteness in the reasoning power and imagination as the reverse, or, at least, a mental condition the opposite of that of suspicion and melancholy (*spes phthisica*). The contradiction here, however, disappears, if we associate the *spes phthisica* and the cerebral and bodily condition of which it is the manifestation rather with the period of developed phthisis, when increased tissue metabolism is more or less present. The physical conditions leading to mental exaltation, as distinguished from depression, are then existent,¹ the "quality" of the nutrition, if we may use the term, is good. Although there is rapid wasting, yet, as was pointed out long ago by Lænnec, there is but slight tendency to septic decomposition and bed sores, and the bodies of phthisical patients are after death slow in running into putrefaction.²

Rheumatism and gout have also been considered in connexion with phthisis, but inasmuch as the diathetic conditions on which the manifestation of these diseases depends often lead to organic cardiac, vascular, or renal disease, we must bear in mind that their effects in this double relation have to be distinguished. That is to say, we have to answer the question as to the effects on phthisis,—first, of the rheumatic or gouty diatheses; and second, of their complications, as cardiac, vascular, and renal disease. The first need only be answered at present.

Rheumatism has been shown to be closely associated with phthisis, either as preceding this disease or as attacking individuals during its course.³ In the phthisical, further, a tendency to rheumatism in the family history is very frequent. It is to be noted, however, that when a rheumatic attack develops during phthisis it is not usually severe, and like all other complications, it seems, at least, frequently to diminish the acuteness of the lung mischief. In such cases, however, or conversely where phthisis succeeds acute rheumatic attacks, although the disease may be chronic, the patient's condition is one of marked feebleness. The gouty habit is by some believed to act as unfavourably towards phthisis as rheumatism;⁴ by others it has been regarded as more or less antagonistic.⁵ In favour of this latter view is its frequent association with adipose tissue, phthisis being not so common in individuals having a tendency to corpulence.⁶

¹ Clouston, *ibid.*, p. 232.

² Pollock, *ibid.*, p. 270. See also Austin, *Lancet*, 1883, p. 10, for cases in which these diseases were associated.

³ Walshe, *ibid.*, p. 450.

⁴ Lænnec, *ibid.*, p. 270.

⁵ Pollock, *ibid.*, p. 273.

⁶ Ebstein on Corpulence, p. 12.

The connexion of syphilis with phthisis has received a great deal of attention, and there can be no doubt that in connexion with the etiology of this disease it occupies a very prominent place. In individuals who are syphilitic by heredity, phthisis is common, as indeed it is among all who succeed to any vitiated constitutional state. There can be no doubt, also, that those who have acquired that disease frequently become phthisical, when in the absence of syphilis they would probably have escaped. It has been said, however, that pulmonary phthisis is rare in the acquired as compared with the inherited form of that disease. As to why syphilis predisposes to phthisis little need be said. We have only to remember the impaired constitutional vitality which it necessitates, and the fact that at any rate in the acquired form it is frequently associated with bronchial catarrh.¹

In this connexion we have also to remember that true syphilitic disease of the lung and respiratory passages occurs, as distinguished from the phthisical process. This is most frequent in the acquired form, but from the fact that it has been found in the fœtus, it may be concluded that it also can occur hereditarily.²

(To be continued.)

Part Second.

REVIEWS.

Von Ziemssen's Handbook of General Therapeutics. Vol. I. *On the Dietary of the Sick, and Dietetic Methods of Treatment*, by Professor J. BAUER. *On the Koumiss Cure*, by Dr STANGE. Translated from the German by EDWARD F. WILLOUGHBY, M.B. (Lond.) London: Smith, Elder, & Co.: 1885.

THE scientific treatment of such a subject as the dietary of the sick is surrounded by difficulties which are well-nigh, if not wholly, insurmountable. A knowledge of the digestive processes under normal conditions may be fairly accurate so far as it goes, but anything like an exhaustive knowledge of the manner in which the multifarious articles comprised in man's dietary are utilized for the repair of waste and the production of energy is still in the future. In the province of disease the problems presented are still more difficult: first, there is the special waste occurring in individual morbid affections; second, there is the altered state of the digestive apparatus, dependent on the primary affection, and

¹ Walshe, *ibid.*, p. 230.

² For references to the works of Schnitzler, Fournier, and others on this subject, see the *London Medical Record*, 1878, p. 206, and 1880, p. 70.

modifying or even annulling normal function; while thirdly, there is the practical problem, What is to be given to replace the waste, and how will the condition of the digestive functions influence and determine the result of our best intentions? Our knowledge, or rather our ignorance on these questions, is not more Tartarean than our ignorance of all departments of pathological chemistry, but it makes any effort at *scientific* dietary almost grotesque. Still a classified statement of ignorance is often of greater value and contains more hope than a classified and fossilized statement of belief. It at least makes the impalpable palpable.

It is evident that it is not without some sense of this that Professor Bauer has fulfilled the task laid upon him by Von Ziemssen. So far as the task was capable of being done well it has been. The investigations and observations of others are referred to and examined, and when possible utilized. The empirical knowledge, which is the growth of experience, has due respect paid to it, and there is no tendency to disregard its dicta, as is so frequently the tendency in *scientific* therapeutics.

The importance of the culinary art is not forgotten.

Koumiss cures, milk and whey cures, grape cures, dry cures, starving cures, and other will-o'-the-wisps which have sparkled across the path darkened by disease and ignorance, and have given to some few new hope, and to some even new life, are found here, and are discussed with a solemnity which might pass for a manifestation of northern humour.

The translator has, on the whole, done his work satisfactorily, and the volume has a useful and an efficient index.

Von Ziemssen's Handbook of General Therapeutics—Vol. II. Antipyretic Methods of Treatment, by Prof. C. von Liebermeister. Antiphlogistic Methods of Treatment, by Prof. Th. Jürgensen. Epidermic, Endermic, and Hypodermic Administration of Medicines, by Prof. A. Eulenburg. Translated by MATTHEW HAY, M.D., Professor of Medical Jurisprudence, University of Aberdeen, London: Smith, Elder, & Co.: 1885.

THIS volume comprises the three monographs specified above.

The first is in many ways the most important, and may be taken as a full exposition of the cold-water treatment of pyrexia as carried out in Germany. Although "the systematic cold-water treatment of fever" seems to have been introduced in the latter half of last century by an Englishman named Currie, Prof. von Liebermeister has to mourn that in "the native land of Currie the majority of physicians appear still to cling so firmly to their old prejudices and the convenient routine which has prevailed hitherto, that they have not been able for once to resolve to give this treatment a trial." The learned professor hardly puts the case correctly. On this side of

the North Sea we are probably prepared to admit that the ardent advocacy of the treatment by our brethren in Germany has had its effect on us; but we have not yet reached that stage of belief which would lead us to cold-bathe our patients as soon as they develop a temperature of 102° . We use the cold bath in hyperpyrexia, and with excellent results often. We also use it, although more reluctantly, in cases where the persistent elevation of the temperature seems to directly and markedly affect the patient's condition, and where antipyretic medicines cannot be given. Further than this we have not yet gone, for the simple reason that we have not satisfied ourselves that the dangers of a moderately high temperature are sufficient to warrant us in adopting, as a matter of routine, a method of treatment which is somewhat heroic, and in the eyes of the patient's friends little short of barbarous. We do not recollect having seen any account of an extended trial, in this country, of the cold-water treatment of typhoid fever, and, indeed, the ideas of physicians will have to undergo considerable change before they accept as a dogma that these cases ought to be cold-bathed as soon as the temperature reaches 102° , and yet this treatment shows excellent statistics. Although in some of the aspects of this question we are not with our German brethren, in others we so entirely agree that it would sometimes seem as if our respective positions might be stated thus: the German view—bathe, but use your discretion not to bathe; the English view—do not bathe, but use your discretion to bathe.

Whatever our views may be, the monograph is well worthy of perusal, for it contains many valuable directions, suggestions, and indications as to the use of the cold bath. Medicinal measures for the lowering of body heat are also considered. Here, also, there is a like advocacy of heroic measures by the administration of very large doses.

The second monograph is devoted to a lengthy discussion on bloodletting, which, it would seem, still has its votaries on the other side, and to the important subject of transfusion. We should have been glad to have found the question of bloodletting in renal affections discussed here.

The third monograph records the state of knowledge on the subjects with which it deals.

Professor Hay, with Dr Barron's assistance, has done his part of the work well, and has added a short appendix on some new antipyretics.

A Guide to the New Pharmacopœia, 1885. By PROSSER JAMES, M.D., Lecturer on Materia Medica and Therapeutics at the London Hospital. London: J. & A. Churchill: 1885.

THIS is an excellent Guide to the New Pharmacopœia. The reader will find in it a clear and succinct description of the various

alterations in this new edition. It contains also a full description of the various new articles of the *Materia Medica*, with their actions and uses. We have pleasure in commending it to the favourable notice of practitioners and students.

Transactions of the Academy of Medicine in Ireland. Vol. III.
Edited by WILLIAM THOMSON, M.A., F.R.C.S. Dublin: Fannin & Co.: 1885.

THIS volume contains a number of papers, some of them of very high value, which have been read in the various sections of the Academy during the Session 1884-5. They are classified according to the section in which they were read. If we are to judge by the number of communications, the pathological section would appear to be the most popular, twenty-three having been made to it, to ten in the medical, twelve in the surgical, and seven in the obstetrical. Of these twenty-three among the more interesting are Mr Stoker's case of Multiple Exostoses, Mr C. B. Ball's of Tar Cancer, and Mr Story's of Epithelioma of the Eyelid resulting from Irritation by Crude Carbolic Acid. Mr Wheeler contributes to the same section an interesting, but clinical rather than pathological, account of a case of Sarcoma of the Anterior Abdominal Wall successfully removed from a girl of 19. Surgeon-Major Hamilton has a long paper on Cholera in the section of medicine, which is further enriched by Dr Walter Smith's article on Lupus, and Dr Cruise's Notes on Contrexeville and Royat-les-Bains. In the surgical section the papers of Mr Ball on Melanotic Sarcoma of the Rectum, of Mr Stokes and Mr Kendal Franks on the Operative Treatment of Flat-foot, and of Mr Thornley Stoker on Internal Urethrotomy, are those which call for most attention. Dr A. H. Jacob has a paper on Compulsory Notification in the sub-section of State Medicine. It is perhaps needless to say that it is adverse, and perhaps as equally needless to add that it is about as unfair a piece of reasoning as ever emanated from the Celtic mind. His statement of the case tried in the Edinburgh Police Court shows how unreliable his facts are.

Handbook for the Instruction of Attendants on the Insane. Prepared by a Sub-Committee of the Medico-Psychological Association appointed at a Branch Meeting held in Glasgow on the 21st February 1884. London: Baillière: 1885.

THIS little book, which contains 64 pages, is the joint work of Drs A. Campbell Clark, C. M'Ivor Campbell, A. R. Turnbull, and

A. R. Urquhart, who are all superintendents of Scottish asylums. The proofs had to undergo a good deal of scrutiny and criticism at several meetings of the Northern Branch of the Medico-Psychological Association. The handbook is divided into five chapters, which severally treat of—The Body, its General Functions and Disorders; the Nursing of the Sick; Mind and its Disorders; the Care of the Insane; and the General Duties of Attendants.

After such careful preparation, it is almost superfluous to say that the handbook is well fitted for the object for which it is designed. We are not sure whether an asylum attendant would fully understand the import of all the information it contains; but no intelligent attendant could read it through without learning much, and the more he mastered it the better would he be fitted for his duties. The medical superintendents of asylums will no doubt in various ways have to supplement the information which his employes may derive from the handbook; but we think that if he distribute it amongst his staff, he will have a good deal less to explain. We believe that this handbook has already had a great circulation, and we hope to see it pass through many editions. In some places greater plainness and clearness of language are desirable, the authors not being accustomed to write down to a less educated class of readers than themselves. The advice about the nursing of the sick is excellent, and might be read with profit by attendants in ordinary hospitals.

The Classification and Treatment of Syphilis. By Dr MARTIN G. B. OXLEY. Printed by W. H. Lead, Leicester.

THIS is a short, interesting pamphlet; and while we cordially agree with much the author says, we regret that his researches into medical literature were not extensive enough to show him how old his views on the classification of syphilis really are. Had he read Laycock's "Medical Observation and Research," he would have found that nearly a generation ago that acute observer classified syphilis with the eruptive fevers. Dr Oxley would then have been unable to say that "we do not in any of our schools or text-books class syphilis among the eruptive disorders."

When speaking of prevention, the author mourns over the repeal of the Contagious Diseases Acts, in which we do not sympathize with him; but he goes on to say, "Had the few who snatched this victory from the hands of a poorly attended House of Commons applied their unmistakable persistency and energy to the framing and passing of an Act for the punishment of any person, no matter what their sex, who knowingly communicated syphilis, either by sexual intercourse or otherwise, they would have had my warmest

sympathy, and I feel sure, that of the bulk of the profession." This form of legislation we have always maintained to be the only rational method of dealing with syphilis.

He sums up his opinion on mercurial treatment thus:—"Although I consider mercury to be a most useful medicine in the early stages of syphilis, just as it is in some forms of scrofula, I feel sure that in mercury we have no certain specific for syphilis, as treat the disease as carefully as we may with this drug, we cannot promise our patients immunity from its dreadful sequelæ, unless they are put on the same footing as other invalids." Our experience is, that where the early eruptive symptoms are left alone by withholding mercury, there are much fewer late symptoms. Dr Oxley strongly advises patients in early syphilis not to do their everyday work as if nothing had happened, and cites the following case:—"I can well call to mind a member of our own profession in this city, who, during the time he was a sufferer from this disease (*caught in the performance of his hospital duties*) carried on his practice with a sore on his hand, and even later on when the rash and other symptoms made it too certain that he was inoculated. He became ataxic, and eventually died from ascending mischief in the cord. He was under most skilful treatment after the orthodox method."

Acne: Its Etiology, Pathology, and Treatment. A Practical Treatise based on the Study of One Thousand Five Hundred Cases of Sebaceous Disease. By L. DUNCAN BULKLEY, A.M., M.D., Physician to the New York Skin and Cancer Hospital, etc. London: J. & A. Churchill: 1885.

IN this, which must be regarded as a companion volume to his treatise on Eczema, Dr Bulkley has presented a most exhaustive study, not merely of acne in its more narrow acceptation, but of the disorders of the oil glands of the skin in general. The extreme frequency and obstinacy of these affections would in themselves have been an amply sufficient reason for its publication, but in addition it is well to have in a compendious form a *résumé* of the mass of literature which has accumulated in connexion with sebaceous diseases. Under acne the author includes seborrhœa, which he describes under the term of acne sebacea, as well as milium and wens, or sebaceous cysts, called acne punctata and acne molluscum respectively. This materially enlarges the sphere of the work, and increases its value. If there is nothing very strikingly novel in the contents, every subject connected with the diseases embraced therein will be found fully and clearly discussed, the diagnosis and treatment being particularly well defined and expressed. The diet and hygiene of acne are handled in a separate chapter; and, being considered from an American point of view,

some of the details are at first sight a little amusing to us on this side of the mill-pond. Thus Dr Bulkley is emphatic in his denunciation of candy, evidently a favourite sweetmeat in the States; while the abuse of iced water is also strongly animadverted against. On the other hand, there are some admirable suggestions as to the mode and time of employing milk, which will be found well worth consideration. A complete bibliography terminates the work, which is well printed, and contains a number of original illustrations.

Journal of Cutaneous and Venereal Diseases. Edited by H. G. PIFFARD, M.D., and P. C. MIRROR, M.D. Vol. III. New York: W. Wood & Co.: 1885.

THIS Journal maintains the character it has borne since its commencement, and a number of valuable contributions have appeared in it last year. Among these we may specially mention that by Taylor on the Pigmentary Syphilide, illustrated by an excellent chromo-lithograph; Dermatological Notes by Fox and Hardaway; Aleppo Button by Altounyan; and Hyde on the Relations of Lupus to Tuberculosis. The selections embrace most of the communications of interest which have been published elsewhere, and Cavasfy and Brocq have furnished *résumés* of the work done in Britain and France respectively. It well deserves the support of the profession.

Transactions of the American Dermatological Association. Ninth Annual Meeting. New York: 1885.

THE subjects under discussion during last meeting were, as a rule, less interesting than in previous years. One of the best was on electrolysis as applied to the removal of port-wine mark and superfluous hairs, but the experience of the members varied greatly as to the amount of result obtained. The general opinion seemed to be, that with hairs this was satisfactory in a large percentage, in port-wine mark uncertain, and often not permanent. The opinion of the members was unfavourable to the value of the oleates, even the oleate of mercury appears to have been overrated, and all declared the oleate of copper comparatively or quite useless in ring-worm. The Association otherwise maintains its position as to membership.

An Index of Surgery. By C. B. KEETLEY, F.R.C.S., etc. Third Edition. London: Smith, Elder, & Co.: 1885.

THE production of three editions in almost the same number of years is in itself a strong proof that this book is a good one, and meets a want. The proof of the pudding, etc.

Fractures and Dislocations. By T. PICKERING PICK, F.R.C.S., etc. London: Cassell and Co.: 1885.

THIS little book does not come up to the standard of some of Messrs Cassell and Co.'s publications. In the first part, where Mr Pick deals with the subject of fractures in general, his remarks are fragmentary and deficient, and decidedly not up to the pathology of the day. For instance, there is no reference to the diagnostic significance of pain and swelling. Again, in speaking of comminuted fractures, where a fragment is completely separated, displaced, and threatening to break the skin, Mr Pick does not even suggest that it might be removed with antiseptic precautions. He considers it would be unjustifiable to convert a simple into a compound fracture for any reason apparently (page 46). Indeed, Mr Pick seems ignorant of antiseptics and Sir Joseph Lister's labours altogether. He makes no mention of antiseptic treatment of compound fracture. Where has Mr Pick been living for the last five and twenty, or, more nearly, thirty years? On the more recent procedure of wiring the fractured patella, Mr Pick is consistently old-fashioned. He considers that any deformity that may result would not justify a treatment (quoting the words of Mr Holmes) "fraught with such grave danger to limb and life." We agree with Mr Pick so far as that most patellar fractures do not require wiring. But we believe that the operation of opening into a joint, *with antiseptic precautions*, is not "dangerous to limb and life."

On fat embolism, or oil embolism, as we think it ought to be called, Mr Pick is unnecessarily brief (page 73).

Mr Pick tells us that the object before him has been to make the manual concise and practical. In regard to the former he has been successful apparently, and perhaps also in the latter object. But we cannot see what practical purpose has been fulfilled in the production of this book. As a guide to students it is not perfect, and they would get all that Mr Pick says in almost any work on general surgery. The book does not pretend to be either scientific or exhaustive, and is insignificant compared with F. H. Hamilton's work.

Surgical Diseases of the Kidney. By HENRY MORRIS, M.A., M.B. F.R.C.S., etc. London: Cassell & Company: 1885.

THIS addition to the collection of admirable works recently published by Cassell & Co. is well worthy a place among those we have already noticed. Mr Morris has provided us with a full and complete account of the surgical affections of the kidney and the various operations which are performed on that organ. We notice with pleasure a commendatory quotation of some valuable experiments

made by Dr Alexander James of this city, on the effects of frequent micturition in damming back the urine in the ureters, and so tending to produce hydro-nephrosis, as in cases of phymosis, stricture, and enlarged prostate. In his remarks on catheter or urethral fever, Mr Morris quotes from the report in our columns of the discussion which took place in the Edinburgh Medico-Chirurgical Society in 1884. He refers especially to Dr P. H. Watson's speech. We are much astonished that having read that discussion Mr Morris finds nothing to say on the subject of aseptic catheters, except to quote the words of a professor of surgery who said, "that a catheter before being used should be boiled, then put into cotton wool while it cools, and then anointed with carbolic oil." We consider it most unfortunate that Mr Morris should have settled on this remark, and have set it forth as the type of "how to make a catheter aseptic." Mr Morris may sneer at a remark thrown out as a suggestion in the course of a speech, but if he in his practice ignores the employment of antiseptic precautions in the use of catheters he will find out his mistake some day to his great vexation. In other respects we highly approve of Mr Morris's book, and strongly recommend it to practical surgeons.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXV.—MEETING V.

Wednesday, 3rd February 1886.—Professor GRAINGER STEWART, *President,*
in the Chair.

I. EXHIBITION OF PATIENTS.

1. *Professor Annandale* showed—(1.) A WOMAN, æt. 22, illustrating the successful treatment of a cirroid aneurism by a combination of electrolysis and ligature of the chief artery passing to the tumour. The aneurism involved the left upper eyelid, and passing from it over the left forehead and temple were several large, tortuous, dilated, and strongly pulsating arteries. One application of electrolysis and the ligature of the temporal artery completely cured the case. Mr Annandale remarked that he had successfully cured several cases of cirroid aneurism by electrolysis alone, but that this was the second case in which he had employed ligature of the artery in combination with it. In the other case the tumour was much more extensive, and he ligatured the common carotid, and also employed electrolysis with complete success.

(2.) A BOY, *æt.* 10, whose left tibia had grown three-quarters of an inch longer than the opposite one in consequence of stimulation of the lower epiphysis, owing to inflammation and necrosis of the shaft of the bone. As a result of this lengthening of the tibia, the foot had been gradually displaced outwards, so as to form a kind of talipes valgus. In order to remedy this deformity it was necessary to lengthen the fibula, and he did this by dividing the fibula at the junction of its lower and middle thirds. In addition, division of the peroneal tendons was required before the foot could be brought into position. The result had been most satisfactory, and the deformity was now quite relieved.

2. *Dr Cotterill* showed A YOUNG GIRL, aged 14, from whose trachea he had some five months previously removed a fibroma. The tumour, which was about the size of a small nut, grew from the posterior wall of the trachea at a distance of an inch and a half from the lower margin of the thyroid cartilages. The tumour was an example of an exceedingly rare condition, very few of such tracheal tumours having been diagnosed during life. *Dr Mackenzie Johnston* had discovered the growth, and had sent the patient to *Dr Cotterill* for treatment. As a paper was shortly to be read on the subject, *Dr Cotterill* merely pointed out the highly satisfactory result that had followed the removal of the growth, as the patient had entirely lost the aphonia and dyspnoea from which she suffered before the operation, and she had also improved greatly in general health.

II. EXHIBITION OF PHOTOGRAPH.

Dr MacGillivray showed for *Dr Nasmyth* of Cowdenbeath the PHOTOGRAPH OF CHILD, *æt.* 3, upon whom a double amputation of the thigh and leg had been successfully performed for injuries received, by the child having been run over by a railway train.

III. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr P. H. Maclaren* showed an ILEO-CÆCAL INTUSSUSCEPTION taken from a strong male infant nine months old, who died forty-eight hours after the first symptom of obstruction presented itself. Death was not a direct result of the disease, but quickly followed, from failure of the heart's action, an attempt to reduce the displaced bowel by the pressure of a column of water, for which purpose the child's hips were elevated and the trunk lowered. Besides the usual symptoms an oval tumour was detected in the left ileo-lumbar region, movable towards the middle line, and painful when handled. The abdomen was neither distended from meteorism nor collapsed, but distinct flattening was noticed in the right iliac region. The preparation illustrated the very formidable changes sometimes attending displacement of the bowel in very young children, the rapidity with which they were accomplished,

and the utter hopelessness in the circumstances of any remedy, medicinal or operative. The intussusceptum was composed of the ascending and transverse colon with the greater portion of the ileum and their mesenteric connexions, all of which were greatly swollen and congested. The mass reached to within two inches of the anus, and was tightly screwed into a spiral shape, and terminated in a bulbous extremity in which the ileo-cæcal opening could be observed. In the concavities of the spiral the mucous membrane was closely pressed into firm transverse rings, and on the convexities the surface was smooth, dark red, and sprinkled with small coagula. The opening in the intussusception was reduced to a narrow vertical slit, but neither there nor in the bowel above, nor in the tumour, was there any sign of adhesive inflammation, corroborating the statement of Treves (*Intestinal Obstruction*, chap. viii.), that that process is never met with before the third day. The irreducibility of the mass was thus solely due to the constriction of the neck, the enormous congestive and œdematous swelling of the displaced bowel, as well as its rigid spiral form, occasioned by the dragging of the mesentery, which is so perilously loose in young children. The examination of such a specimen should tend to make the prognosis guarded, even when laparotomy is performed at an early stage of the disease.

2. *Dr Cotterill* showed a PAPILLOMA OF THE LARYNX of considerable size. This case had also been sent him by *Dr Mackenzie Johnston* for treatment. The tumour was pedunculated and grew from the right vocal cord. For three years the patient, a woman of about 60, had suffered from huskiness and attacks of dyspnoea; and these symptoms had progressively increased in severity till the date of the operation. Intra-laryngeal treatment having been found impracticable, *Dr Cotterill* had removed the papilloma by slitting through the cricoid cartilage and crico-thyroid membrane, pulling down the growth from above, and twisting it off. The patient had made a perfect recovery, and at present her phonation was almost normal, while her attacks of dyspnoea had entirely disappeared.

3. *Dr Littlejohn* showed—(1.) A PIECE OF SKIN from inside of leg of a man, æt. 65, who had a varicose ulcer of old standing the size of a florin. He was a wood turner by trade, and as he was working at the lathe hæmorrhage occurred from the ulcer, and in the absence of assistance death ensued. Two hours had elapsed since he had been last seen. The specimen showed that one of the dilated veins opened on to the surface of the ulcer. (2.) A RIGHT FOREARM AND HAND in which an ordinary tableknife was firmly clenched. Deceased evidently committed suicide when insane, as marked evidence of chronic cerebral disease was found on dissection. The death had been slow, resulting from syncope from gradual loss of blood, and the entrance of blood in limited

quantity into the air-passages. It was pointed out that where sudden syncope occurred from a large and rapid hæmorrhage the weapon dropped from the hand; but when, as in the present case, no large vessels were wounded, death took place slowly, and the weapon was found firmly grasped. (3.) A LOOKING GLASS bespattered with drops of blood, presenting all the characters of having escaped from a small artery. Deceased had inflicted the fatal wound standing in front of this glass.

IV. EXHIBITION OF INVENTIONS.

1. *Dr Foulis* showed an INSTRUMENT which could be used either as a stomach pump or as a means of sucking up fragments of a crushed stone from the bladder after lithotomy. It consists of an ordinary aspirator, the limbs of which are large and at right angles to each other. To the horizontal limb is attached a short vertical tube with a stop-cock. By means of a funnel, which can be easily attached to this tube, the stomach can be flushed with liquid, or an antidote to a poison may be caused to flow into the stomach, or the bladder may be constantly filled with an antiseptic lotion, all of which fluids can be immediately aspirated or withdrawn by a single pull on the syringe attached to the aspirator.

2. *Dr Allen Thomson Sloan* showed a NEW DIABETIC LOAF made from "Crude Gluten," manufactured by the "Health Food Co.," New York. He said that he would like to bring before the notice of the Society what he might call a new "diabetic loaf." For the past few months he had had occasion to diet a friend of his suffering from diabetes, and for the first time, he thought, thoroughly realized the great difficulty of supplying a truly satisfactory substitute for bread. He first tried the various gluten preparations made by Sawers & Mackie of this city, and afterwards those manufactured by Messrs Van Abbott of London. These not only proved unpalatable, but produced an utter distaste for all other kinds of food. His attention was then directed to the "Crude Gluten Flour," prepared by the "Health Food Co.," New York, and from this the loaf he now exhibited was made. Though not absolutely free from starch, it contained less starch than any other bread allowable in diabetes. It was highly palatable, and, at the same time, very nourishing. Professor Austin Flint had used it in many cases with the most satisfactory results, and the experience of the "Health Food Co." extended over many thousand cases, to only a very few of which it had to be denied. He had used it regularly for the last four months in the case of a patient, aged 60, where the sugar, on two occasions for a week or so, had entirely disappeared, and where a careful quantitative analysis from almost daily observations had at no time showed any increase. Even more

palatable than the bread were the scones made from this flour. These were best made with sweet milk and ordinary baking powder, and fired in a very hot oven. They were much better than those made from pure gluten flour, which had a bitter taste, and soon became very tough. His object in showing these preparations to the Society was to induce those Members who had diabetic patients, either in private or in the wards of the Hospital, to give them a fair trial, when he was certain, that far from proving injurious, they would be found to be as beneficial as they were sure to be welcome to the self-denying sufferers. The bread and flour were obtainable from Mr James Clark, 30 Greenside Street, Edinburgh.

V. ORIGINAL COMMUNICATIONS.

1. *Professor Annandale* read a paper on COMPRESSION OF THE INNOMINATE ARTERY, WITH NOTES OF A CASE. He considered (*First*), the temporary compression of the innominate artery in order to stay hæmorrhage from the right subclavian and first part of the right carotid during operation, or in connexion with injuries involving these vessels. He described a simple and efficient method of compressing the innominate artery by making a central cervical incision, such as is employed in low tracheotomy, and then gently inserting the finger or the blade of a compressor behind the artery, and compressing it against the sternum or sterno-clavicular joint. He referred to experiments which he had made twenty years ago in connexion with some of Mr Syme's operations upon axillary aneurism, and he showed an instrument which he had had constructed at that time for compressing the artery. (*Second*), The question of compressing the innominate artery more permanently for the relief of aneurism of the right subclavian or right carotid arteries. He related the notes of a case in which he had endeavoured to cure a subclavian aneurism by compression in this way, but owing to the continuous pressure of a drainage-tube upon the artery (innominate), ulceration took place, and fatal hæmorrhage resulted before the treatment had been properly carried out. He specially directed attention to this unexpected risk from the pressure of a drainage-tube lying in a wound communicating with a large artery, and although this case had proved fatal, he was still in hopes that some modification of the treatment suggested might be an improvement in relieving aneurism upon the right side of the neck,—more particularly as ligature of the innominate artery had been proved to be so unsatisfactory a proceeding.

The President remarked upon the practical interest and value of such communications as Professor Annandale's.

Mr Bell endeavoured to account for the rapid production of fatal ulceration by the drainage-tube, on the theory that great arterial expansion of vessels of neck and aneurism would follow the exposure of them by the operation from the removal of the sup-

port naturally given by muscles and fasciæ, and related some illustrative examples.

Dr MacGillivray mentioned a case confirming the observations made by Mr Annandale, in which the pressure of a drainage-tube had led to secondary hæmorrhage from the popliteal artery, necessitating amputation, in a case of excision of the knee-joint.

Dr McBride hoped that the President and Members of the Society would excuse him for rising to discuss a matter of which he knew so little. Still he could not refrain from asking, entirely for his own information, whether it was not an old and respected axiom in pathology that constant pressure caused atrophy, while intermittent pressure tended to produce hypertrophy. He thought that this was the accepted explanation as to the occurrence of corns and other callosities on the skin. If this were so, was it not probable that the continuous pressure of a drainage-tube upon an artery would tend to produce perforation of its walls?

Dr P. H. Maclaren failed to detect any novelty of principle or advantage in the process of treating aneurism which Mr Annandale had just described. The danger arising from the pressure of a drainage-tube on the vitality of the coats of an artery has been long recognised even in situations where the counter pressure could never be so great as in the case under discussion. In amputations Mr Spence carefully adjusted the drainage-tube so as not to touch the ligatured vessel. Pressure always is the invariable prelude to ulceration, the rapidity of its occurrence and its extent being proportional to the amount.

Mr Cathcart pointed out that special experience with particular compressing agents, drainage-tubes or otherwise, seemed unnecessary when the general law of absorption from constant pressure was so well understood. He instanced the effect of a soft mucous polypus in the nose producing the so-called expansion of the nasal bones,—a process which in all probability was really an absorption of the original bone on the nasal aspect, with a corresponding periostitic new growth on the opposite or facial side.

Dr James remarked that although in the case of a normal artery exposure by the opening up of the surrounding parts would probably not render its walls more likely to give way, yet if it were dilated, or if there was on it an aneurism, so that the opening up of the surrounding tissues allowed it to become more distended, and consequently increased the area of its internal surface, there would, according to the hydrostatic law, then be a greater risk of its rupture.

Dr Foulis said that as aneurisms of the larger bloodvessels were so constantly associated with an atheromatous condition of those vessels, he could not help thinking that at the spot where the burst took place there must have been a small spiculum of calcareous matter, or an atheromatous condition of the inner and middle coats of the bloodvessel. The indiarubber drainage-tube

lying against the bloodvessel at this spot would act as a foreign body and cause an alteration in the structure of the external coat. As the result of the irritation, the fibrous elements would be converted into the looser cellular elements, and the bloodvessel would necessarily be reopened at that point. In addition, the greater intravascular pressure, after the operation of ligature, would subject this spot to a greater strain than the weakened external coat could bear, so that at last a sudden movement on the part of the patient would cause it to burst. He had difficulty in believing that a healthy large bloodvessel would give way under similar circumstances.

After a few remarks by *Mr A. G. Miller*,

Dr Littlejohn regretted that the specimen connected with this case had not been exhibited, as many of the points alluded to by the speakers, and which had given rise to discussion, would at once have been cleared up; although the interesting debate, to which they had all listened with pleasure, would, in all probability, not have taken place, and thus the Society would have been the loser.

Dr Cotterill remarked that the proposition made by *Mr Joseph Bell* (to the effect that the hæmorrhage was induced by the altered blood pressure consequent on the removal of the support afforded by the fasciæ, etc., which had been cut through in the operation of exposing the innominate artery), was one which, if correct, should to a certain extent modify our action in exposing any large vessel for the purpose of ligature or for other reasons. As far as he (*Dr Cotterill*) knew it was not a risk which was usually appreciated as such by surgeons. He asked whether it was not possible that the removal of the slight support afforded to the innominate by such fasciæ and cellular tissue was not compensated for by the more direct atmospheric pressure exerted on the vessel after section of the skin and other tissues overlying the vessel.

2. *Dr M'Bride* read a paper on UNCOMMON MORBID CONDITIONS OF THE EAR, NOSE, AND LARYNX, which will appear in a future number of this Journal.

The President, in thanking *Dr M'Bride* for his communication, expressed his satisfaction at seeing that the character of the work of Edinburgh specialists continued to be so high. He remarked that the paper gave evidence of general medical culture as well as of special knowledge; that he had heard of some specialists whose work was like the lights used in their rooms,—very bright on the individual spot, but surrounded by general darkness. *Dr M'Bride* represented that good school of specialists, who could throw as intense a light upon the special subject as any one, and yet had a good illumination all round.

Dr James said that among the large amount of material of great practical interest which this paper contained, what seemed to him

to be of particular interest was the association of a peculiar laryngeal lesion with the development of phthisis. That in a case where this apparently slight laryngeal mischief existed, the onset of pulmonary tubercle at no distant date might certainly be predicted, was the opinion of laryngologists, but he should like to hear Dr M'Bride's opinion of the connexion. Supposing that the laryngeal lesion were tubercular, the development of pulmonary symptoms might reasonably be explained by resorption; but in the absence of such a tubercular focus, could not the more or less incessant coughing tend to bring about apex mischief?

Dr Mackenzie Johnston thought the paper one of great interest both to the specialist and to the physician. The importance of recognising the symptoms of tinnitus and auditory vertigo was evident, for the physician frequently met with such cases. He had himself recently seen a lady who had been under treatment for a long time in England for gastric derangements, where the cause of the distressing symptoms, he found, was due to chronic catarrh of the middle ear, which yielded to appropriate treatment. Referring to the laryngeal case, he said, that from the drawing he recognised it as a case that he had seen with Dr M'Bride. He had had no difficulty in diagnosing the inter-arytenoid growth, but not then having heard of Stoerk's observation, he had not known its significance. The case was a corroboration of a most important observation, and was a further proof of the assistance that medical diagnosis can receive from the use of the laryngoscope. As to the cause of this growth, he should like to suggest that (as touched upon by Dr James) the cause might be due to the irritation and constant movement of the inter-arytenoid fold in coughing. An argument in favour of that would appear to be the frequency of tumours in the larynx, while in the smooth, inactive canal of the trachea they were extremely rare.

Dr M'Bride begged to thank the Society for giving him their kind attention. Owing to the late hour, he would endeavour to condense his reply as much as possible. Dr Duncanson had expressed a desire to hear his views of Dr Cooper's theory. So far as he remembered, this author believed that most of those cases which aurists look upon as due to chronic catarrh were really due to a vascular dyscrasia,—"basic aural dyscrasia," according to Dr Cooper. Dr M'Bride frankly admitted that he had been unable to follow the arguments which led to this view. If he remembered rightly, one of the chief arguments used in the paper was the frequent presence of a subclavian bruit in the class of cases referred to; but how easily could such a murmur be produced by a little additional pressure upon the stethoscope! In some cases anæmia existed—as he had shown in his paper this evening—associated with chronic middle ear catarrh, and might be of itself a factor in causing tinnitus. In such cases, of course, iron was extremely useful. Dr James had asked the question

whether in such a case of inter-arytenoid tumour as the one described, the pulmonary mischief might not be caused by the act of coughing. Dr M'Bride hardly thought this likely. At the same time, it was but quite recently that the first autopsy of a case of primary laryngeal phthisis had been made. During life it was, of course, impossible to be certain as to the absence of deep-seated, small, caseated nodules. He did not think that the connexion between inter-arytenoid tumour and phthisis had yet materially advanced beyond the stage of clinical observation. It must also be remembered that in phthisis, and with rare exceptions, if any, in phthisis only did a true tumour appear in this situation. With regard to the nasal tumour, he would like again to call attention to the fact, that had the tumour been attached by a shorter pedicle to the lower part of the posterior nares, the operation would have presented little difficulty. It was the fact that the neoplasm was inserted in the most inaccessible part of the nasal cavity by a long pedicle that made its removal only possible by means of traction with the finger. Moreover, at one period of the operation, the growth had got impacted in the left nostril in such a manner that it could only be felt after it had been pushed back again with a gum elastic catheter; this fact alone gave some idea of its mobility. Dr Johnston had referred to hydrobromic acid in tinnitus, as recommended by Dr Woakes, especially in the pulsating variety. Dr M'Bride thought his experience as to its negative value tallied with that of many other observers.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XLVII.—MEETING II.

Wednesday, 9th December 1885.—Dr HALLIDAY CROOM, *President, in the Chair.*

(Continued from page 876.)

Dr Underhill thought Dr Barbour had done well in separating the scientific from the practical side of the question under discussion. Further, he was very glad of the opportunity we had had of listening to the exposition of the scientific aspect of the question by Dr Hare. We were often too apt to overlook the importance of the scientific views of such subjects. It was long since Sir James Simpson showed the relations of the stump-like surface of the uterus post-partum to this disease, and Semmelweis insisted upon the danger of the dissecting-room poison in the lying-in room. It was not, however, until Pasteur brought the demonstration of the causation of this condition clearly forward, that the treatment based on this demonstration was regularly adopted. The result was that after antiseptics came to be systematically applied we had an enormous fall of the mortality from this cause, a

greater fall of death-rate than had been effected by any other means. The success which followed the adoption of perfect cleanliness gave the key to the pathology of the conditions. It seems to him the prevention of puerperal fever was a point demanding greater attention at the present time than its cure. We should go to our patients in a scientifically clean state, and see that all our surroundings are clean. We should bear in mind the polymorphic character of the poison we have to deal with, a poison which may come from scarlet fever, from erysipelas, from a defective drain, and from other sources. The cause of the autogenetic form corresponds to the form of organism found in dead tissue. If we remove the source of the infection by washing out the uterus in these cases the symptoms disappear more rapidly than those arising from any other cause. He believes, accordingly, that this variety of septicæmia is much more amenable to treatment than that arising from general septic poisoning. In this case, if we remove the cause we remove the effects. When, on the other hand, the poison comes from the outside the case is different, and the treatment is vastly more difficult. Thus it comes to be most important to attend to prevention; and when we do meet with a case treatment ought to be prompt. He agrees with the others who have spoken as to the value of corrosive sublimate, and would resort to it in all such cases.

Dr Foulis hesitated to say anything on a condition about which, as it seemed to him, so little of what is really certain is known. He came to the meeting with his mind filled with doubt as to the nature of the disease which we have been discussing, and he was bound to say that nothing which had been said as yet had served to clear up any of those doubts. And so it seemed to him that, not knowing the nature of the disease, it is premature to ask, How can we apply a remedy? Are we to conclude that this new organism is the only cause of puerperal fever in all parts of the world? Is puerperal fever a name given to that form of blood-poisoning arising after parturition only, or is it a specific fever? There are numerous forms of the fever, as *Dr Underhill* has pointed out. The symptoms are endless—neuralgia, fever, etc., etc. If this micro-organism then is the cause, is the puerperal state the only one in which it can develop? Is inflammation the process which produces the *nidus* in which this micro-organism can develop, or is the inflammation the *result* of the development of this micro-organism? If we regard the condition of the puerpera we see that the blood is circulating in all directions, and that a poison introduced into the blood gives rise to the fever, etc., which we call blood-poisoning. If we are correct, then, in attributing the symptoms of this disease to the circulation of this poison in the blood, how, he would ask, can we explain the disappearance of these symptoms which, it is claimed,

so often follows the local application of antiseptic douches to the uterus? He has abandoned the idea that puerperal septicæmia is a specific disease at all, but believes that there may be many sources of infection. Further, the poison may even be absorbed by the lungs, as in breathing an infected atmosphere tainted by the emanations from a water-closet or drain. So far as the specific cause is concerned we are, it seems to him, in the dark. But not so much in the dark as we would be did we set aside the germ theory of the disease. That the condition arises from the introduction of germs he thinks admits of no doubt, and this ought to give us the clue to our treatment. Thus we should see that no woman has a child in a bad atmosphere; we should see that no "vaccine" material be introduced by the hand of the doctor or the nurse. He regards the prevention of puerperal blood-poisoning by a preparatory treatment of the patient by means of certain drugs as a suggestion of a line of treatment which may prove of the highest importance. He thinks the treatment by the washing out of the uterus by means of antiseptics after the development of the fever is too late. Why not wash out the uterus as soon as the child is born, so as to prevent the development of the disease at all. To him it seems that to wait until the fever has developed is very much like locking the door after the steed is stolen.

The President, in closing the discussion, thanked Dr Hare, a stranger, for the valuable contribution he had made to the discussion, and at the same time took this opportunity of conveying to Dr Barbour, on behalf of this Society, its thanks to him for originating the discussion, and opening it by a contribution of such scientific and clinical importance. The whole subject was admittedly a difficult one, but he thought that discussion had done much to clear the ground. For example, take the question of washing out the uterus with some antiseptic lotion in cases of puerperal septicæmia. Of the value of this there could be no possible doubt. He had himself, both in hospital and private practice, met with cases post-partum and post-abortion where marked septicæmia had set in with a high temperature, and where, after one or two washings of the uterine cavity with 1 in 2000 solution of perchloride of mercury, the temperature had fallen, the symptoms of septic poisoning disappeared, and the patient made an excellent recovery. Of the value of intrauterine antiseptic irrigation in cases of autogenetic septicæmia there could, to his mind, be not the faintest doubt. Still, till now, there was the difficulty in explaining this action. Dr Barbour had shown to-night that micro-organisms require rest for their development. They will not multiply in the circulatory fluid. The fact that they do not multiply in the circulating blood explains why the washing out of the uterus acts beneficially. It cuts off the supply of microbes, and as those already formed do not increase in the moving blood, their ultimate elimination is only a matter of strength

and time. As to the value of antiseptics in obstetric practice nothing could be more remarkable than his own experience in the Edinburgh Lying-in Hospital. Entering on duty in an entirely new hospital, in his first quarter he met with an outbreak of puerperal fever, in which he lost three successive cases. He then had introduced careful antiseptics, drawing up strict rules for nurses, pupils, and resident-surgeon, and from then till now, a period of six years, he had not lost a single patient from that cause. Whatever theories might be urged to explain it, the fact remained that the adoption of antiseptics had practically stamped out the scourge of puerperal fever, not only in the Edinburgh Hospital, but at Prague, in London, in the pavilion Tarnier in Paris, in Copenhagen, and indeed, wherever this treatment had been honestly and carefully carried out. He congratulated the Society on the discussion, and believed that whatever might be its intrinsic value as an addition to our scientific knowledge of the subject, and he believed this was great, if it pressed home on the Fellows the importance of the most minute antiseptic details in obstetric practice, the evening had been well spent.

SESSION XLVII.—MEETING III.

Wednesday, 13th January 1886.—Dr HALLIDAY CROOM, President, in the Chair.

I. *Professor Simpson* showed for Dr Robertson Crease a HYDRO-CEPHALOUS FŒTUS. A. B., æt. 36, married. Family history, good. Father died of apoplexy, aged 70 years, and mother about the same age, of bronchitis. On the father's side, history good; no history of any nervous or brain disease. Menstruated at age of 14. Type, 28 days; duration, 3 days; amount abundant and florid. In intra-menstrual period slight leucorrhœa. Has had nine children at full term, all healthy at birth. Her second child, a boy, at age of $2\frac{1}{2}$ years, had at attack of acute polio myelitis anterior, and is living. Her first child, female, was still-born, no motion having been felt by the mother for some days previous to birth. The mother is of medium height, well developed. Pelvic measurements—Interspinal, 10 inches; inter-crystal, $11\frac{1}{2}$ inches. Her last menstrual period ended on 2nd March 1885, and she was delivered on 4th January 1886. She states that she has been more or less in pain since 25th December, and that labour began by the escape of the waters on 1st January. I was sent for on 4th January in the afternoon, and found, on examination, the pelvic cavity was filled by a tense fluctuating mass, which, on careful examination, was found to consist of head of child, fontanelle of which were enormously expanded, and the bone thin. Pains were vigorous, and by this time, and when the head had fully distended the perineum, fearing that it would not pass without the size being diminished, preparation was made for

perforation. A drachm of extr. ergot. liq. was administered. The next pain was sufficient to expel the mass *en bloc*. The placenta was expressed, and the patient has recovered without a single bad symptom. Out of 3476 tabulated cases which have occurred in my practice, I have only met with three dropsical cases, viz., the above-mentioned hydrocephalic, one case ascites, and one hydrothorax. The two latter had to be delivered by forceps after much difficulty. Length of child, $22\frac{1}{2}$ in.; circumference at chest, $13\frac{1}{2}$ in. Head diameters—occipito-frontal, 6 in.; occipito-mental, $6\frac{1}{2}$ in.; sub-occipito-bregmatic, $4\frac{1}{2}$ in.; biparietal, 5 in.; bitemporal, $4\frac{1}{2}$ in.; trachelo-bregmatic, $4\frac{1}{2}$ in. Circumferences—occipito-frontal, 17 in.; occipito-mental, $17\frac{1}{2}$ in.; sub-occipito-bregmatic, $15\frac{1}{2}$ in. The chief points that call for special notice are—the duration of pregnancy, the length of the first stage of labour, the size of the head, and the peculiar way in which it was projected in an elongated form over the perineum, and no rupture of uterus or perineal tear taking place.

II. *Mr Skene Keith* showed an OVARIAN TUMOUR, which had been characterized by marked slowness of growth, taking $4\frac{1}{2}$ years to attain its size at the operation. The cause of this tardy growth seemed to be a twist on the pedicle. The tumour was firmly adherent, and on this account the direction of the twist could not be certainly determined. Though the operation had been attended with considerable difficulty on account of the adhesions, the patient had done well.

III. *The President* showed (1) for *Mr Lawson Tait* TWO PAIRS OF OVARIES AND TUBES, which he had removed from patients of his (the President's) during a recent visit. The one patient was suffering from a very large bleeding fibroid, which had already (three weeks since the operation) become considerably reduced in size. The patient has otherwise done remarkably well, and was then sitting up for some hours each day. The other ovaries were removed from a woman who, seven years ago, had been delivered of a child after a prolonged and instrumental labour. Since then she has led a life of almost continuous and intense suffering, so that she has been entirely unfit for her domestic duties. There was intense dysmenorrhœa and dyspareunia. The ovaries were matted down in the pouch of Douglas, and exquisitely sensitive. The patient was now completely free of pain, and was discharged that morning, twenty-five days after the operation. (2) A PHOTOGRAPH of enormous enlargement of the mammæ in a woman, aged 19, in the fourth month of pregnancy.

IV. *The President* read a NOTE OF A CASE OF RETENTION OF URINE FROM AN UNUSUAL CAUSE, which appeared at page 734 of this Journal.

Professor Simpson regarded this as an important clinical observa-

tion. He had frequently seen laceration of the perineum giving rise to this condition, but retention from the cause recorded was new to him.

Dr Milne Murray said that the symptoms of this case and those arising from laceration of the perineum recalled a classical experiment of Goltz. On sponging the anus of a dog with warm water, micturition almost invariably followed. This can always be instantaneously stopped by pinching the skin or mucous membrane in the vicinity of the anus with forceps. No doubt the afferent stimulus reaches the corresponding centre in both cases, and induces spasm of the sphincter. An observation which he himself had made recently seemed to point to an identical mechanism in the rabbit. In the research on which he was at present engaged on the uterus of rabbits, it was necessary from time to time to empty the bladder, which filled rapidly, apparently under the action of the anæsthetic as a diuretic. Evacuation of the bladder was usually effected by applying the electrodes from the induction coil to the fundus of the bladder. On the occasion referred to, he found that the strongest current available failed to cause emptying of the viscus, and resulted merely in an irregular vermicular contraction of the walls of the bladder. Subsequently, when the bladder had become enormously distended, a canula, which had been fixed into the rectum by a ligature passing through the tissues round the anus, was removed, and the moment the ligature was cut and the compressed parts released, the bladder emptied itself with great force. This seemed to him an interesting observation as confirming the view that the retention following laceration or injury of the perineum was a purely reflex result, and was in no way caused by a dread on the part of the patient of allowing the urine to flow over the raw surface. Goltz's experiment on the dog and his own observation on the rabbit seemed to him to place the reflex character of the condition beyond doubt.

Dr Symington read a paper ON THE POSITION OF THE UTERUS AND OVARIES IN THE CHILD, WITH REMARKS ON THE GROWTH OF THE FEMALE GENITALS, which will appear in a future number of this Journal.

Professor Simpson wished first to thank Dr Symington for bringing this valuable paper before the Society. It was one of great importance, and would greatly add to the value of the *Transactions* of this session. He had followed the paper with much interest, more especially that portion of it which dealt with the position of the ovaries. The position of these organs has always been somewhat indeterminate. Various opinions have been held as to their position. This discrepancy was probably due to the change which occurred in their position and relations in the course of their development, and it seemed to him quite likely that some of the observations had been made on ovaries whose development had somehow been imperfect, and whose

position would thus differ from that of the fully developed ovary. That part of the paper which dealt with the uterus was likewise of much value, not only with reference to its direction, but also with regard to its position and relations. He hoped Dr Symington would pursue this investigation, and tell us more about it. It was interesting to note the point of the reflection of the peritoneum from the anterior aspect of the uterus to the bladder. This has been regarded by some as coinciding with the os internum. If this were true in these cases, then the os internum must be at the fundus uteri. The change in the direction of the inclination of the three canals—urethra, vagina, and rectum—was also worthy of note. While in the infantile state all three were parallel and were directed forwards, the rectum came in the adult state to alter its direction and run backwards.

Dr Hart was much indebted to Dr Symington for the privilege of hearing this valuable paper. The observations had been characterized by great accuracy. He was sorry to say that this accuracy did not, as a rule, characterize the work of anatomists on the pelvic viscera, and gynecology had suffered accordingly. Before the important bearings of a paper such as this could be fully appreciated one must have it in print, but one or two points of special interest struck him while listening to it. 1. The relation of the bladder in determining the position of the uterus. This was a point which had been much overlooked, and he was glad to observe the importance given to the connexion. The drawings shown illustrated the well-developed state of the bladder during early childhood. 2. The presence of the *arbor vitæ* on the whole cavity of the cervix and uterus. This observation was new, and seemed to him of great interest. 3. The relations of the ovary and the Fallopian tube. He agreed with His in describing the Fallopian tube as arching over the ovary, and this view is confirmed by Dr Symington's preparations. In conclusion, he wished again to express the pleasure he had derived from the paper.

Dr Foulis was able to support Dr Symington's views as to the position of the ovaries, from a prolonged series of observations he made some years ago. Dr Foulis then sketched the process of development of the ovaries from the Wolffian bodies, and showed how their position depended on their development from this structure. From the relations of parts, their position would at first necessarily be vertical, but this position would be gradually modified, and become nearer horizontal as the development of the organs proceeded.

The President proposed, and Dr Craig seconded, that the Society cordially thank Dr Symington for his valuable paper, and for his trouble in bringing the beautiful preparations and drawing illustrating it before them.

Dr Symington acknowledged the compliment.

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING IV.

Thursday, February 11, 1886.—Dr LOCKIE in the Chair.

Dr Lediard showed a patient recovering from COMPLETE ALOPECIA OF THE SCALP. The woman had been under observation for two years. It commenced with one patch, and in a few weeks all the hair disappeared from the scalp, eyebrows, and eyelids. It was associated with headache. No history of syphilis. After six months hair began to grow scantily over the scalp. For a year there was a scanty growth of white hair, and during the last few months pigment has appeared in the hair, and the hair has grown more in patches. Blisters, spirit lotion, and repeated shaving have constituted the local treatment; while quinine and iron, bromide of potassium, and iodide of potassium, have been administered internally.

Dr Barnes had a case of universal alopecia, associated with diabetes mellitus and exophthalmic goitre.

Dr Lediard showed a patient with BLACK TONGUE. The pigment was insoluble in chloroform, alcohol, ether, and liquor potassæ. The papillæ were elongated, hypertrophied, and pigmented.

Dr Maclaren showed a patient, aged 23, with a rapidly growing MALIGNANT TUMOUR OF THE RIGHT UPPER EXTREMITY. It followed an injury to the right elbow seven months ago. There was also a lump under the left scapula, and enlarged glands in right axilla and above right elbow.

Dr Lediard showed a MYELOID SARCOMA OF LOWER JAW, along with a MICROSCOPICAL SPECIMEN.

Dr Maclaren read the following NOTES ANENT PESSARIES:—

The possession of the specimen which I show has induced me to lay before you some very decided opinions I have on the subject of pessaries. It was obtained from a woman, aged 73. Thirteen years ago she suffered from prolapsus uteri, for which she consulted a doctor. He introduced the instrument before you. A month afterwards his attendance ceased. For eleven years the pessary caused little trouble, but she then began to suffer from vaginal discharge, vulval irritation, and a much impaired state of general health, without being able to state any definite ailment. I first saw her six months ago, and she told me the above story. An examination ascertained that the vagina was packed full of a gritty, putty-like substance, having a horribly adipocerosus odour, in the centre of which was the pessary. With some trouble I removed the instrument. I was not able to reach the os with

my finger, and her feeble condition precluded much interference. She was feverish and ill for some time, and then gradually pulled together. She is now in fair health, but very frail. I have not felt justified in making another vaginal examination, and the uterus has not again come down. I have no doubt that the pessary was made of gum-elastic, and was pear-shaped. It can still be made out that the covering is a textile material, and that it is filled with tow.

This is the fourth instance that has come under my notice in which a pessary has been abandoned and left derelict in the vagina. In two of the cases they were wooden ball pessaries, and did no great harm beyond causing some vaginal ulceration. In the other case a mushroom-shaped wooden pessary split, owing, I think, to some crack in the wood which moisture increased. A part of it found its way into the bladder, and was drawn back by the road it went.

I once also operated on a vesico-vaginal fistula, which followed the use of a pessary for prolapse. The instrument had been carefully fitted, and has since been worn with safety, the only difference being that the patient now removes it nightly.

I now, therefore, come to my first note—*Always teach the patient to remove and introduce her pessary, and reject all pessaries which do not allow of this.* I am sure that every useful purpose can be served by an instrument which may be taken out each night and replaced in the morning. I do not very often use pessaries, but still I see sufficient uterine disease to come across most varieties of it. And one of three pessaries meet all my requirements,—a watch-spring, a Hodge, or a Zwank. There is no difficulty whatever in getting a patient to use these in the way I have stated. Of course women are teachable in different degrees, and it requires more perseverance with some than others, but I have never ultimately failed. I think I ought to note one possible exception to the above general statement, that is in the use of the intrauterine stem pessary. As I have no experience of its use, I content myself with the statement, that if it is useful and to be used, the patient cannot introduce or remove it.

My second note is—*Always have the pessary made of a material impermeable to moisture.* Soft, pure rubber or vulcanite are the best. Leaving in diluted Condyl's fluid over night will make such perfectly clean and odourless. A pessary which is at all absorptive becomes very foul and offensive.

The third note is—*Get your patients to soap all soft rubber pessaries.* Oil spoils the rubber, making it soft and porous.

My fourth and last note is—*Be sure that a pessary is needed.* No doubt pessaries have been used too frequently. There are women who seem to derive much mental and physical comfort from having their vaginal canals turned into marine stores for some variety of curious little instruments such as I show you [a large collection of various pessaries shown], and find rest in the assurance

of a displaced womb which has been put right. Now, before advising the use of a pessary, we should be sure that there is a mechanical basis for the symptoms which a mechanical remedy will rectify. The fact of a uterus being in some other than the typically normal position is not in itself a reason for using a pessary.

Dr Lockie read a paper on the ETIOLOGY OF ACUTE GLOSSITIS:—

By acute glossitis the author meant the so-called idiopathic form, excluding such inflammations as may be obviously produced by mechanical injuries, burns, scalds, or chemical irritants. The idiopathic form is admittedly rare.

The disease has been attributed to a great variety of causes, such as the excessive use of mercury, stings of wasps, cold, septic poisons. It is also said to occur as a complication of certain fevers. The author considered that cold, at most, could be only a predisposing cause. Many diseases, as erysipelas, formerly ascribed to cold, are now known to have a septic origin. He believed that acute glossitis was due to specific morbid agents. The swelling of the glands, the more or less definite duration, and the tendency to spontaneous recovery, seemed to support this theory.

He then narrated a case in which the poison of erysipelas had seemed to cause an attack of glossitis. The erysipelas attacked, firstly, the fauces, then the tongue, and lastly the skin.

GLASGOW OBSTETRICAL AND GYNÆCOLOGICAL SOCIETY.

SESSION I.—MEETING IV.

Wednesday, 13th January 1886.—Dr W. L. REID, President, in the Chair.

THE main business of the meeting was the discussion of Dr Samuel Sloan's paper, "On the Treatment of Labour Delayed by Obstruction at the Pelvic Brim," which had been read to the Society on the 11th November last. There was a large attendance of Fellows, and the discussion, which lasted for about three hours, was a very animated one.

Before beginning the discussion, Dr M. Cameron moved that, as some addition had been made to Dr Sloan's paper, the discussion should be limited to that portion of it which had been read before the Society. No one having seconded this motion, the discussion was allowed to proceed.

Dr Robert Park said that he thought the members of this Society would have no difficulty in taking the pamphlet which Dr Sloan had been so kind as send round to them as being practically the embodiment of what he read to the Society two months ago, and he thought it would occur to every member of this Society that Dr Sloan had not only done himself credit in bringing forward this subject in the way he had done, but that he had also conferred dis-

tion upon the Society. It was not every man who could have brought forward this subject in the way which Dr Sloan had done, even although he had the ability and the practical experience it requires in addition. They had to remember the position Dr Sloan held at the Maternity Hospital. But there had been many who had held that position during the last twenty or twenty-five years who had not taken in hand to contend with the maxims and aphorisms which the late Sir James Simpson so ably pled and worked for. He thought that the position taken up by Dr Sloan, in what he thought was a most important part of the paper—namely, the mathematical part—was very strong. He demonstrated very clearly the double wedge form of the foetal head. It had long appeared to him that the teaching of Sir James Simpson upon this subject was unsound; and especially that his teaching with reference to pelves contracted at the conjugate diameter had been fraught, not only with very great mischief for the children that were to be born, but also with danger to the mothers who were bearing these children. So far as his own individual experience went, he said it had been very unfortunate in the matter of turning in cases where there was disproportion. Out of several he instanced three cases in his practice where version had resulted in very difficult extraction, and in death to the children. In one case, in which he had been engaged with other two medical men, rupture of the uterus took place. He, in conclusion, agreed with what Dr Sloan had said, and thought that the ten propositions set forth by Dr Sloan at the end of the paper were all very just, and would go a long way towards guiding practitioners in dealing with these cases.

Dr John Glaister said that he felt he would have the assent of every member of the Society when he said that a more excellent paper on a difficult subject had not been presented to any society for a considerable time. As the author had remarked, no subject in the large number connected with obstetric work had been more copiously discussed than this one; and it was to be hoped that the paper under discussion might contribute to its settlement. The object of the paper was to find out the safest form of delivery both for mother and child in a flat pelvis, whether it be a simple flat pelvis, a generally contracted pelvis, or a contracted flat pelvis. In dealing with a case of this kind, it seemed to him that the first point to be attended to was to estimate as truly as possible the measurement of the conjugate diameter; and in this there was met the first difficulty, because of the imperfect methods at our disposal. Dr Glaister instanced a case which happened in the Maternity Hospital during the year 1881, and which is detailed in volume vii. of the *Edinburgh Obstetrical Transactions*. In that case five members of the Hospital staff came to the conclusion that the conjugate measured $3\frac{1}{4}$ inches, and version was decided on. The woman died on the eighth day, and at the post-mortem examina-

tion the true conjugate was found to be only $2\frac{1}{2}$ inches. This was not an unusual experience. He thought that Dr Berry Hart's paper on the "Causation of the Movements of the Head at the Brim of the Rickety Pelvis" should have been alluded to by Dr Sloan. Dr Hart details the movements of the head in the contracted pelvis when left to nature, and points out that the head passes the conjugate by its bitemporal diameter, or one slightly behind it. A considerable degree of importance must, he thought, be attached in the mechanism of such a case to the diameter to which Dr Sloan had given the name of *lateral conjugate*, which gives increased room to the extent of a quarter to half an inch. He now felt that he could see the value of it in connexion with the mechanism as described by Dr Hart, where he said that, when the forehead dips and the head is shoved as a whole to the occipital side of the pelvis, the biparietal diameter of the head would then be engaged, in what Dr Sloan calls the lateral conjugate of the pelvic brim. Now as to the treatment. The diversity of opinion upon this subject seemed to him to be due to the difficulty of the position. Some practitioners preferred turning to forceps as an initial step, others *vice versa*, and these two schools have each, in trying to prove the value of their treatment, led proof to substantiate their position. He did not require to travel over the ground again. However, there was no doubt that the Edinburgh School believed in turning as an initial choice. What the precise teaching of the Glasgow School was he was not prepared to say, because it had not yet been sufficiently crystallized. In Dr Sloan's paper, however, the tendency was to approve of the forceps as a preferable operative measure to turning, and the facts and cases adduced had all tended in this direction. He stated that Dr Hart in his paper had given the history of twelve cases, in all of which turning was employed, the true conjugate varying from 3 to $3\frac{1}{2}$ inches, whilst in one it was $2\frac{1}{2}$ inches. No mother was lost, and nine of the children were born alive. In two of the cases, where living children were obtained, all the previous children had been delivered still-born by forceps. Dr Glaister then gave details of several cases of delivery (which occurred in his own practice) of living children by forceps in contraction at the conjugate diameter. In one case he had employed pressure on the abdomen, as recommended by Dr Sloan in his paper in the *Glasgow Medical Journal*. Dr Glaister here introduced the two living children mentioned in Case II. of the paper. Dr Sloan, in common with many other Members, examined the heads of the children. In the case of the girl, Dr Sloan had seen, examined, and measured her head-mark a few days after birth, but the indentation was now so small that he quite failed to recognise it. In the case of the boy with a larger head, the indentation was fairly easily detected. Dr Glaister thought the fifth argument was the crucial one. Herein lay the key of the position—the solution of the case.

So far as his experience went it did not show that version could be more easily performed than the application of the forceps. This was the rock upon which the different schools split. Indeed, in these cases in which version had been successfully performed, *quæ* the child (say Tarnier's famous case, *Obstet. Soc. Ed. Trans.*), the mother died from the indirect effects of the operation, as also in the case before mentioned (vol. vii. of same *Trans.*) The only thing that enables one to form an opinion on this point is experience, and the amount of success attending the operation, whether turning or forceps. If it could be shown that in a certain number of cases with an ascertained conjugate, version produced better results than forceps, or *vice versa*, then they would have something definite to work upon. But even here factors must be taken into consideration which could not be neglected, viz., the size of the child's head and its condition, *quæ* ossification, and the skill of the operator. This is well evidenced in the case of Mrs W., Case IX. of the paper. He maintained that there should be, before turning is tried, fair grounds for believing that the child is alive, and will be brought alive through the narrow pelvis. In regard to Case XII.—that of Mrs M'G.—he denied that turning had been attempted by him before the woman was sent to the Hospital. He quite agreed with Dr Sloan in thinking that it would have been better to perforate at once. In conclusion, he said it should always be understood that the interests of the child ought always to be secondary, and that in every instance the mother's life ought to be the first and main consideration.

Dr M. Cameron quoted copiously from Otto, Spiegelberg, Michaelis, and Dohrn; and stated that, in his opinion, they all agreed that the diameter of the child's head which passed the conjugate in head first cases was the bitemporal. The explanation of Dohrn's illustrative case, mentioned by Dr Sloan, was that the child was premature, and the pelvis was very small. He was unable to give Dr Sloan any other measurement of that pelvis than the true conjugate, which was 7.8 cm. He stated that some additions had been made by Dr Sloan to his paper out of books which he had given Dr Sloan in loan. He failed to see how forceps with a pelvic curve could permit of the movements of the head described in Case III.

Dr T. F. Gilmour said Dr Sloan's paper was very long and elaborate, and so much in it was open to criticism that one's remarks must be rather disjointed. He thought it nonsense to discuss whether we should save the mother or the child. We should do our duty to both equally. We were under no contract to deliver the mother out of her present difficulty, but only to bring all our knowledge, skill, and experience to her aid and to that of her unborn child. He inclined to use version in cases of doubt, and said, if we go back to Smellie we find him using it frequently and successfully. He ridiculed the argument quoted from Dr

Matthews Duncan as being *post hoc*, and therefore valueless. He instanced a very difficult case in which Dr Reid succeeded in performing version at the Maternity Hospital, after two of the outdoor physicians had failed, under chloroform. He suggested that those who were skilful in turning would be likely to prefer it, and that perhaps Dr Sloan's skill in the use of forceps led him to prefer them. As to the mechanism of labour in contracted brim, he thought there was nothing new in the paper, nor in the discussion so far—it was all to be found in Schroeder's text-book. He considered that what Dr Park had termed the "mathematical part" of the paper was of great importance. He failed to see how any one could confuse the terms bitemporal and bimastoid, and thought the former a more scientific and accurate term than coronal. He denied Dr Sloan's statement that we could only wait five or six minutes after the birth of the body, if the child is to be saved by version, and quoted two of Dr Sloan's own cases where living children were born after "seven" minutes, and after "about ten;" also cases from Smellie of twenty and even thirty minutes, and yet the child had been born alive.

Dr A. Wallace said that as Dr Sloan had in the first page of his paper invited criticism, he had pleasure in expressing his opinion upon one or two points in the paper. Dr Sloan had attacked the teaching of Sir James Simpson, but he (Dr Wallace) wished the Society to remember—and here he did not speak as a partisan—that the original paper was written in 1847, and that a great advance had been made in the literature of this subject during the last fifteen or twenty years, especially by German investigators. He was inclined to think that the cases brought forward in Dr Sloan's paper were not observed with sufficient care by Dr Sloan himself, nor exact pelvic measurements given, to warrant him in formulating his conclusions. Dr Sloan stated that he had seen many cases of obstruction due to contraction of the conjugate diameter of the pelvic brim, yet we find that a large proportion of the cases here recorded were observed by others. Dr Glaister has, I think, three out of the ten cases (excluding Dr Turner's carefully observed case). Dr Wallace endorsed the able criticism of Dr Glaister, but thought that he had just stopped at an important point, namely, the indications for treatment. In observing the presentation and mechanism of cases of retarded labour in the milder degrees of antero-posterior contraction of the pelvic brim, you may have an indication of the treatment required to assist in completing delivery. If you can easily reach the small or posterior fontanelle lying on a lower plane of the pelvis, and the anterior out of reach of the examining finger—indicating that the occiput is coming down first—then the use of the forceps is, I think, indicated; whereas if we make out the anterior fontanelle without difficulty lying at or near the same level as the posterior, version is indicated. As a rule, however, you must judge of each case on its own

merits, for there is the difficulty that we have to deal with—a more or less unknown quantity—the size and degree of compressibility of the foetal head. I think Case IX. teaches a very important lesson. Here we have a slight reduction in the antero-posterior diameter of the pelvis; and on two occasions, after a *tedious* labour, delivery of living children took place, while in four other labours with artificial aid the children are all dead. In the original paper of Sir James the principal question taken up is not so much one of forceps *versus* version, but “Turning as an alternative for Craniotomy.” Dr Wallace mentioned a case with a conjugate of probably 3 inches, in which the “waters” had escaped, with the “balling” of the uterus resulting, and a difficulty in effecting the revolution of the child. He introduced his hand through the os, carrying up a tube and injecting a mild antiseptic fluid into the interior of the amniotic sac—the hand and arm was plugging the os and vagina—when revolution of the child was with comparative ease accomplished. Dr Wallace considered that Dr Sloan had misrepresented Professor A. R. Simpson’s views of the mechanism in cases of contracted brim, and maintained that the quotation from Simpson’s *Contributions* regarding the passage of the parietal bone over the promontory did not indicate that the biparietal diameter of the head passed through the narrow conjugate of the pelvis. In conclusion he had only to say that he thought the Society owed Dr Sloan thanks for his paper.

Dr John Ritchie said that he was in a position to know that Dr Sloan’s paper, as now in the hands of the Fellows, was practically the paper that had been read to them two months ago. Regarding what had been said about Continental views, Dr Sloan’s object, so far as he knew, was the guidance of practitioners in this country. He believed that where forceps failed, craniotomy should be the next operation. In his experience, the *data* given by Dr Sloan are correct.

Dr Robert Bell said that in the earlier years of his practice he was closely associated with the late Dr J. G. Wilson, and thus had many opportunities of observing the effects of turning in cases of contracted pelves, and he declared that the results had been very unsatisfactory indeed. In later years he had invariably had recourse to the forceps; and he would say that the chances in favour of living children were very much greater with the forceps. One point to be remembered in the case of small pelves was, that the head of the child might be very large, and this was sometimes due to the women having gone beyond their time. In such cases it is very desirable that labour should be brought on at the expiry of forty weeks.

Dr Robert Pollock said that Dr Sloan had given a most valuable contribution to the Society on a subject which had occupied the attention of the most eminent obstetricians. But as regards the treatment of such cases, we were left much in the same

position as when we started the discussion. The difficulty was the accurate gauging of the pelvimetry, and we could not apply a hard and fast rule to such cases. That there were facilities in the delivery of head last cases was undeniable, and was proved by the fact that we found in the same woman, with a breech presentation, a well-formed rounded head, and again, with the head presenting, there was a marked cone-shaped elongation of the presenting part. He had seen good results from turning in cases of deformed pelvis. But if turning was decided upon, it must be done early, before the membranes were ruptured; and having diagnosed deformity he would not wait until full dilatation of the os, but proceed to dilate it. Waiting implied rupture of the membranes, contraction of the uterus on the child, and consequently, if turning was then attempted, increased dangers to mother and child. He did not think that craniotomy on the after-coming head (if turning then failed) was more difficult than in head presentations. He questioned the statement, that rapid dilatation, if gently performed, caused rupture of the cervix. Rupture of the uterus was, however, caused (and he had a pathological specimen of that lesion) by attempting to turn at a late stage, and he considered that to have attempted turning in cases like XII. was foolish in the extreme.

Dr Alex. Miller said he did not intend to criticise the paper, but merely to give the result of his own short experience. He gave the result of eighteen cases where the labour was delayed by obstruction at the brim by narrowing of the conjugate diameter. In these cases there were different degrees of contraction; and he wished just to state what his practice had been, and what the results were. An analysis of the eighteen cases showed that seven were primiparae, in all of whom the forceps were used to effect delivery, with the result of one maternal death and two children. In this case, where the mother died, death resulted from peritonitis on the eighth day; and in this case also, though there was a narrowing of the conjugate diameter, it was not so marked as in the other cases; but there was considerable rigidity of the maternal structures. Five of the cases were secundiparae, in three of whom the forceps were used, and in two version with podalic extraction, with the result of the death of one child only, and that a case of version on account of arm presentation. Two were confinements for the third time, in one of whom forceps were used, and in the other podalic version, where all did well. Three were confinements for the fourth time—all forceps deliveries, where the mothers did well, but with the deaths of two of the children, because of their size. One was a multipara, having had more than four children, in whom forceps was used to effect delivery, when both did well. As a rule *Dr Miller* prefers to deliver by the long forceps in these cases of obstruction at the pelvic brim.

Dr J. K. Kelly felt that there was a great array of authorities quoted at the beginning of the paper, but the result at the end was very small. But he had only had time to glance at the paper. What had surprised him very much was that in the whole discussion that had taken place no notice had been taken of the great dilatation of the pelvis which took place in these cases. He was convinced that the pelvis was distended during the passage of the head. This dilatation was so marked, that it could be seen even when the woman was lying under the bed clothes. One did not require a microscope to see it. He preferred forceps to version, because, in his experience, it was as difficult to bring the head after the rest of the body was born as it would have been if the forceps had been applied to the head before turning. In a few cases of funis and shoulder presentations he had turned, but he was always glad to find the head at the brim. His mortality had been low. He had had about 3000 cases of labour, and in all these cases he never had to perform craniotomy.

After some remarks by *Dr James Ross*,

Dr Geo. Halkett said that his experience was somewhat similar to that of *Dr Kelly*. When he had a case of head at the brim he always used the long forceps. He had also had about 3000 cases of labour, and had never performed craniotomy *in his own practice*.

Dr Gilmour asked if it was not possible that the protracted and laborious use of the forceps might cause idiocy in the child.

Dr E. H. L. Oliphant said there was a recent article in an American journal where cases were given of idiocy after version.

Dr J. Stuart Nairne said that his mortality had been very small. He had only once to perform version and once craniotomy, and these were for other practitioners. He was of opinion that some practitioners met with contracted pelvis more frequently than others.

Dr David Tindall said—In cases of obstruction at the pelvic brim, with a normal presentation (*i.e.* cranial), I prefer, and invariably do employ the forceps in preference to turning, and have seldom been what I would call very successful with turning after the forceps had failed.

The President said that the forceps had this advantage, that they caused flexion of the head as far as this can be, and he thought the size of the pelvis was not the only thing to consider; the size of the child's head, and still more its compressibility, were of equal importance. His practice had been to try the forceps; and if, after fair trial, they failed, he then considered the propriety of turning, and performed this operation if the woman was in a fair condition.

Dr Sloan thanked the Society for the measure of praise they had bestowed on his efforts. He was glad to find that *Dr Park* agreed

with him in preferring forceps to turning in the cases under consideration. When a man deliberated for years on any subject, and then declared that he had come to certain conclusions, he thought we were bound to respect his opinion. When any one informed us that his practical experience over a course of years had led him to adopt a particular line of treatment, we naturally looked on him as speaking with authority. But when any one, from combined theoretical knowledge and practical experience, informed us that he had come to look on version as a dangerous substitute for forceps and craniotomy, as Dr Park has done, we were forced to reconsider our position. Dr Glaister had suggested that some information regarding the mechanism in contracted pelvis, when Nature had been able to overcome the difficulty, might have been given in the paper. His account of this mechanism from Dr Hart's paper was very interesting, and Dr Sloan had great respect for anything written by Dr Hart; but, at the same time, he had been obliged to curtail his paper, and had trusted to the members of the Society either knowing the general principles of this mechanism already, or finding it elsewhere. He thought there must be something wrong in the figures quoted by Dr Glaister with regard to the case published by Dr Turner. The correct measuring of the conjugate was, however, a real difficulty till a man had acquired considerable experience in it. He felt very strongly the importance of Dr Glaister's remarks regarding the value of experiments on the passage of head first and head last cases through the same conjugate whose size was known, and agreed with his statement that then we would have something definite to guide us. Now, it so happened that he (Dr Sloan) had brought with him to this meeting translations of extracts from Dr Budin's thesis (*"De la Tête du Fœtus"*), which had been kindly sent to him by the author since this paper was read to the Society.

At page 100 Dr Budin writes thus:—"To sum up, all our experiments with foetal heads at term were made in pelves whose conjugates were 7 to 8½ cm. (2¾ to 3⅜ ins.) In experiments 1 to 7 (see table), in which the foetuses were drawn through by the feet, there was separation and laceration of the cervical vertebræ as well as almost constant 'squashing' ('enfoulement') (driving in) of the parietal bone which was in relation to the promontory. On the contrary, the head could be drawn through with the forceps without causing lesion of the skull.

"In experiments 8 to 10 the head came through as easily coming last as after being seized with the forceps, but traction always required to be greater after version. More than that, in one case the parietal bone was driven in; and in that case traction had arrived at that degree at which, in other experiments, the vertebral elements had given way.

Finally, in experiments 11 and 12, traction on the foetus com-

ing footling was less than that exerted on forceps. But each time, in that case, considerable driving in occurred of the posterior parietal bones, which seemed more yielding and easier to depress than they usually are.

"Therefore at term the forceps is to be preferred in the contractions of $2\frac{1}{4}$ to $3\frac{1}{8}$ ins., which we have been considering.

"Finally, in contractions below $2\frac{1}{4}$ ins. version does not seem to give the possibility of the extraction of a foetus at term either alive or 'viable.'"

On the contrary, in experiments made on foetuses born before term, especially towards the seventh month of uterine life, with a pelvis measuring $4\frac{1}{2}$ to 7 cm. ($1\frac{3}{4}$ to $2\frac{1}{4}$ ins.), we have got the following results:—

"Traction is always less after version and much greater with forceps, and the application of the forceps seems to succeed only if the head is in the transverse position and is seized flexed in a fronto-occipito direction.

"We must therefore agree with Barnes, that in cases of great contraction delivery by version should be the complement to the induction of labour at seven or eight months.

"These remarks refer only to flat pelves."

At page 102 he continues,—*"Mechanism in head last cases, head in transverse diameter, first flexes, then descends."*

At page 104 he says,—*"If you continue to pull in head last cases, the bitemporal diameter, which is engaged in the conjugate, diminishes, but all the other diameters are able to increase—the antero-posterior diameters as well as the vertical. On the contrary, if the head presents it is generally seized from forehead to occiput, and when you engage it, the reduction of the bitemporal diameter is much less easy, for the head, being engaged on four sides, can increase only in its vertical diameters. Hence there is necessity for stronger traction after application of the forceps."*

At page 106 he remarks further,—*"We have seen the cervical ligaments give way with a strain of 55 to 66 lbs. (25 to 30 kilog.)"*

"These figures are considerably less than those recorded by Joulin, or even by Matthews Duncan. We used foetuses immersed in hot water for an hour or two, and the dynamometer was tested just before and after each experiment."

He (Dr Sloan) was quite willing to accept Dr Glaister's statement that he had not attempted turning in Case XII. before the woman was admitted into the hospital; but he was satisfied as to the substantial accuracy of the report, and still felt that the advice given by the late Dr J. G. Wilson was a reasonable one, considering the state of our knowledge then. Regarding the books on this subject out of the Faculty Library, which Dr Cameron had been kind enough to show him since the paper was read, Dr Sloan explained that when he had carefully examined these works, he found that they contained statements not sufficiently

explicit, and views which Dr Sloan considered erroneous. He had taken note of some of these in his paper, but they had in no way tended to the alteration of views he had already expressed. Regarding Dr Gilmour's remark on Dr Matthews Duncan's view of the result when version ended in the birth of a dead child, Dr Sloan expressed his approval of Dr Duncan's view. Dr Gilmour might fail to see how any one could mistake the bitemporal diameter for the bimastoid, but Dr Sloan could assure him that so careful a writer as Dr Robert Barnes had apparently fallen into this error. Dr Wallace had been surprised that the paper had attacked the position assumed by Sir James Simpson in 1847, and had ignored the fact that some alteration had taken place in the practice of his followers, but Dr Sloan begged to remind the Society that in every English-speaking obstetric school the main contentions of Sir James Simpson were still accepted as true, and practice founded on them was still the rule. They would surely admit, therefore, that it was wise and right to take Simpson's paper as the basis of his own. He admitted that the cases (sixteen in all if the one incidentally referred to at page 649 of the Journal is included), were not all observed by himself. Of the three under Dr Glaister's care, however, one had been carefully measured, and noted at the time by him (Dr Sloan), and another was actually delivered by himself. As to Dr Turner's case, it had been attended throughout by Dr Sloan, though the delivery was effected by Dr Turner. But he (Dr Sloan) might remind Dr Wallace that Prof. A. R. Simpson's important contribution to this subject was written with only *three* illustrative cases. Regarding Dr Wallace's objection on the point of uncertain pelvic measurements in the cases noted, he supposed he must acknowledge himself to be in the same position as other observers in this difficult matter, and simply utilize all his experience so as to make these as accurate as is obtainable. Dr Sloan on his part, however, was surprised that Dr Wallace had failed to notice and criticise that part of the paper dealing with the mechanism of head first cases in a justo-minor pelvis. He had no great name to quote here in favour of his view; but as he considered it of great importance to show that the mechanism was not *necessarily* different in the flat and the generally contracted pelvis, he demonstrated with a justo-minor pelvis and a foetal cranium what he meant to enforce. Dr Wallace failed for some time to see what Dr Sloan meant, but the President, on being appealed to by the latter, acknowledged that he (Dr Sloan) had made it quite plain. Dr Sloan also demonstrated with the same pelvis and foetal head how, by diminishing the transverse diameter of the pelvis by about one inch, the head could be made to pass the brim, so that the promontory would mark the cranium *exactly* as is represented in Dohrn's illustration. As to Dr Wallace's interpretation of Dr A. R. Simpson's sentence as quoted in the paper, Dr Sloan, whilst explaining that the words

italicised are not so printed in the original, felt that if the meaning of the passage was that the parietal bone rounded the promontory with its protuberance to the left of the *whole* of the promontory, the words used in describing this movement could not but be regarded as misleading. But as this was only a question as to the meaning which the author meant to be attached to the words he used, and in no way touched his argument, it was of no practical importance. He would like Dr Wallace, however, to explain how the biparietal diameter could fail to pass through the conjugate diameter if its posterior pole passed over the promontory whilst the head was in the *transverse* diameter of the pelvis. Dr Wallace's reference to the figure in Sir James Simpson's paper illustrating the passage of the bitemporal diameter through the narrow conjugate must have arisen from his failing to see that the head is in that figure represented as passing *base* first. Dr Sloan was glad to find one of so much experience as Dr Bell regarding version in contracted pelves with suspicion; and he quite agreed with Dr Bell that it was *possible* for a woman to carry her child for a longer period than forty weeks. Dr Sloan referred Dr Pollock to the paper, where he would find that both turning and podalic extraction after turning were looked on by him as liable to cause lacerations and ruptures. The President had told them that the flexion which the forceps caused was an advantage, but he would remind the Society that this argument was used by Simpson against the forceps in contracted pelves. He agreed with the President that the size of the head of the child was of as much importance as the size of the conjugate, for it was simply a question of *disproportion* between the head of the child and the brim of the pelvis; and the decided position Dr Sloan took up regarding the question of treatment, *quæ* DISPROPORTION, would be found in his first proposition.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

PURE TEREBENE AND TERPENE.—I have received so many letters from medical men in different parts of the country giving results of their experience with pure terebene in the treatment of winter cough, that, practically, there has been an informal collective investigation. I have before me brief notes of 94 additional cases, and in 71 of these a distinct success is recorded. In 6 of the cases of failure, a further investigation showed that there had been an incomplete diagnosis, the patient suffering from some complication,

such as aneurism or aortic disease, the existence of which had not previously been suspected. In 10 cases the patients complained of nausea after taking the medicine, and in several instances when inhaled from lint it excited the cough, and apparently acted as an irritant. The explanation is that much of the pure terebene now sold is of very inferior quality, and would be more accurately described as "impure" terebene. Medicines, as a rule, do not improve by keeping, but pure terebene is certainly an exception, for as it gets old it becomes bland and oily. It is to be feared that most of the old stock, which, in consequence of the small demand, must have been in the druggists' shops for years, has now disappeared, so that greater care has to be taken in the selection of good specimens. Much of the pure terebene now sold is crude and irritating, and quite unfitted for medicinal use. My attention has been called to the fact that there is a terebene which is labelled "patent." I distinctly stated in my paper that the substance I employ is not a "patent" preparation. In almost every case I find the pure terebene has been given on sugar, and has not been used in the form of spray, probably from the difficulty experienced in obtaining the atomising apparatus. The combination of oil of cubebs, oil of sandal wood, and pure terebene, which I used with success, has given good results in other hands. I have had no returns of phthisis treated in this way, but most of my correspondents speak of having found pure terebene of benefit in acidity, flatulence, and other forms of dyspepsia. The smell of violets, which terebene imparts to the urine, is often referred to, and in one instance it gave rise to complications. *A lady called on her lover, who was ill in bed with bronchitis, and was very angry because he would not show her his violets. She searched for them, but did not find what she had expected. I am surprised that pure terebene has not been recommended for diseases of the bladder and urethra. My experience of its employment in these cases is limited, but I have certainly found it useful in cystitis and gleet. Delicate and fastidious women sometimes object to the smell and taste of pure terebene, and I then use terpene in the form of tabloids with good results, although I do not think that it is equal to pure terebene.* (William Murrell, M.D.)—*The British Medical Journal*, 27th Feb. 1886.

THE EFFECTS OF COCAINE ON THE CENTRAL NERVOUS SYSTEM.—Dr D. R. Brower draws the following conclusions from a paper with the above title, published in the *Journal of the American Medical Association*, 16th January 1886:—1. Cocaine in small or moderate dose is a cerebral stimulant, but produces derangement of the digestive and assimilative functions, and diminishes the elimination of waste. 2. The use of cocaine in the alcoholic and opium inebriates is not satisfactory, while it is a more or less perfect substitute, yet its use is attended with greater danger than

alcohol or opium. 3. The use of cocaine in mental depression, if we carefully guard against the depressing effects of the drug upon digestion and assimilation, will often give better results than any drug hitherto used. 4. The use of cocaine in neurasthenia is a valuable addition to the treatment. 5. The drug, if administered in large doses persistently, causes a very marked deterioration of the central nervous system, producing a profound cerebral neurasthenia, and may produce such a malnutrition of the cerebrum as to develop insanity. 6. Cocaine occasionally in doses heretofore regarded as small, produces alarming depression of the central nervous system.—*The Therapeutic Gazette*, 15th February 1886.

COCAINE IN LARYNX, PHARYNX, AND NOSE.—Dr Dessar epitomizes his experience with cocaine in the larynx, pharynx, and nose, in the following aphorisms (*Deutsche Medicinal Zeitung*, 7th January 1886):—1. Cocaine reduces the sensibility of those parts, even in existence of hyperæsthesia, to such an extent that the laryngoscope and rhinoscope can be employed with ease. 2. Reduction of pain, especially in operations. 3. Removal of pain and difficulty in swallowing in presence of a stenosis depending upon tumours in phthisis, syphilis, perichondritis, and tonsillitis. 4. Ischæmia in case of strongly injected mucous membranes. 5. Diminution of profuse hæmorrhages. 6. Insurance of diagnosis in reflex neurosis of the nose. For purposes of examination a five per cent. solution is sufficient; for those of cauterization of the pharynx, nose, and for the removal of deglutition difficulties, a ten per cent. solution, and for laryngeal operations a twenty per cent. solution are required.—*Therapeutic Gazette*, 15th February 1886.

EFFECT OF NITRITE OF AMYL ON THE BLOOD PRESSURE.—The influence which inhalations of nitrite of amyl exert on the arterial pressure is usually thought to be a lowering of the same. The investigations and direct measurements of Dr Schweinburg (*Centralbl. für Medical Wiss.*, 2nd January 1886), with Basch's sphygmograph, however, brought out different results. The blood pressure was seen to rise after inhalation of the drug, at the time when the well-known symptoms, reddening of the face, sensation of warmth, pulsation of the carotid arteries, etc., set in. This apparent contradiction between the ordinary assumption and Schweinburg's researches, this observer explains by the statement, that nitrite of amyl given in large quantities, or in small but repeated doses, produces a reduction of the pressure, but after small quantities such as are exhibited for the therapeutic purposes a transitory rise takes place. Hence care must be taken in cases in which a sudden rise of pressure would be dangerous.—*Therapeutic Gazette*, 15th February 1886.

A NEW GALACTAGOGUE.—At a recent Medical Congress in Naples, Professor Siurleo called attention to the *Ditana digitifolia*, a

Mexican plant, as possessing remarkable galactagogue properties (*Phar. Post*, 28th November 1885). He mentioned several cases reported in Italian journals in which the administration of an infusion (of what parts of the plant is not stated, nor the strength) to nursing women had exercised a marked influence upon the milk glands, inducing a considerable secretion of milk, even in cases where it had been nearly or entirely suppressed. Ethereal tincture of the flowers of *Ditana digitifolia* made into a kind of elixir with syrup is said to have been brought out by an Italian pharmacist under the name of "Galattofore." The flowers, and especially the stamens are, in addition, credited with sudorific properties. Professor Siurleo has also directed attention to the *Rhamnus alaternus* and *Ligustrum vulgare* as plants possessing properties antagonistic to those accredited by him to *Ditana digitifolia*. The property of *Rhamnus alaternus* in diminishing the secretion of milk was reported about forty years since, though since practically overlooked, and Professor Siurleo now states that he has found the *Ligustrum vulgare*, though so far removed from it botanically, to possess exactly similar properties. The drug is said to be best administered as an infusion of 3 grammes (45 grs.) of leaves in 150 grammes (5 oz.) of water twice in the course of 24 hours; this dose being repeated for several days.—(*Pharm. Journ. and Trans.*, 25th December 1885).—*The Therapeutic Gazette*, 15th February 1886.

URETHAN.—In our December issue we gave particulars regarding the chemistry of this substance, which is the ethylether of carbonic acid ($\text{NH}_2\text{CO}_2\text{C}_2\text{H}_5$). Since that time it has come into notice in this country, and physicians have it on trial. Regarding its physiological action we quote from the *Druggists' Circular* a letter by Merc of Dramstadt, who says: "Schmiedeberg first experimented by using urethan on animals; later, Jolly applied it to human beings, thereby proving it to be a hypnotic. Von Jacksch, assistant of the first Medical Clinic in Vienna, in the last few months subjected urethan to a close examination, which resulted in justifying its classification among the best known hypnotics. Von Jacksch further calls attention to the fact that all other hypnotics produce more or less of other indications causing false impressions, which properties urethan does not possess. Von Jacksch has employed this remedy in various forms of insomnia, in the dose from 0.5 to 1 gramme, even in the higher forms of phthisis, in derangement of the valvular system, and in fatty degeneration of the heart, with the most serious symptoms; in all cases it produced quiet and lasting sleep, without causing any annoying reactions. Concerning its physiological action it should be classified among those remedies acting on the brain. The irritability of the sensitive respiratory organs not being altered to an appreciable extent, it is concluded that this preparation must act without greatly influencing heavy coughing, asthmatic attacks, neuralgic pains, and the lance-like sensations of unde-

veloped consumption. Von Jacksch is convinced that urethan will prove a specific in the treatment of diseases of children, as the sleep produced by it very much resembles natural sleep. Urethan is administered with or without corrective in form of powder or in solution. Here it must be mentioned that urethan in small doses is not an infallible sleep-producing remedy, but its hypnotic effect, after administering it to the extent of one gramme, leaves nothing to be desired."—*The Chemist and Druggist*.

EXTERNAL USE OF LOBELIA INFLATA.—Dr V. N. Reichard highly recommends the use of *Lobelia inflata* (*Med. Times*, 12th December 1885) as a local application for indolent sores, chronic erysipelas, and especially in incised wounds, in which latter class of cases it acts as a hæmostatic and astringent. The mode of employment of lobelia for this purpose in cases of incised wounds, no matter how great the hæmorrhage, provided, however, it does not require a ligature, is to bring the edges of the wound together, and to hold them for a few moments, while a pledget of cotton wet with tincture of lobelia is applied. Dr Reichard says that the hæmorrhage will then cease and the parts adhere, and, although lobelia may not be a germicide, it will so entirely close up a wound as to render it perfectly aseptic.—*Therapeutic Gazette*, 15th February 1886.

PERISCOPE OF MEDICINE, CLINICAL AND PATHOLOGICAL.

By G. A. GIBSON, M.D., and WILLIAM RUSSELL, M.D.

THE INFLUENCE OF FLUIDS (BOISSONS) ON NUTRITION AND IN THE TREATMENT OF OBESITY.—Professor Albert Robin discusses this question. There are two views held: the one, that liquids are inactive; the other, that they are of real influence. He accepts Genth's position—that water introduced into the stomach in large quantity augments the proportion of urea and salts in the urine. But the further question is, Is this due to a more thorough washing of the urea already formed, or to an augmentation of disintegration? He thinks it is partly due to the former; and that the tissues being more thoroughly freed from the products of their disintegration, oxidation processes will be favoured. Forster found not only increase of urea, but also of sulphur compounds, which were doubtless due to the breaking up of albuminoids. The next point to settle is whether the augmentation of the urea depends on increase in organic disintegration or on an exaggeration of combustion. From Genth's experiments and his own he concludes that the ingestion of a large quantity of water augments combustion—that is, oxidation without augmenting equally organic disintegration. Obesity has two causes: in the one, assimilation is augmented; in the second, destruction (*désassimilation*) is diminished. In the first class, water

ought to be strictly curtailed or interdicted; in the second, the ingestion of a considerable quantity of water will entail a marked loss of fat. But the question has to be answered, How can we distinguish these two classes? This is accomplished by finding whether the quantity of urea is increased, diminished, or stationary. An increased excretion of urea shows that there is an increased assimilation, while on the contrary a diminution of urea indicates very clearly a diminished oxidation. An individual makes little urea, either because he eats little or because oxidation is affected. In the first case, he eliminates as much nitrogen and carbon as he ingests, and he does not, therefore, become fat; in the second, the balance is in favour of ingestion, and he fattens. But the more numerous cases are those in which the urea is normal. In these cases, instead of estimating the urea, it is necessary to estimate the co-efficient of oxidation; this is raised when obesity is from excess of assimilation, lowered when from *désassimilation*. It therefore follows that fluids ought to be freely administered in obesity when there is a small excretion of urea, or when the co-efficient of combustion is diminished; but fluids are to be abstained from in those who secrete much urea, or in whom there is an increase in the co-efficient of combustion. He illustrates the above contentions by some successful cases.—*Gazette Médicale de Paris*, 30th January and 6th February 1886.

CASES OF ANKYLOSTOMATOSIS.—Collard relates some cases of this interesting affection from the English Hospital. During 1885 they had several patients suffering from the symptoms of pernicious anæmia, in whom ferruginous preparations produced no amelioration, and in whose motions, on microscopic examination, were found the eggs of the parasite *Ankylostomum Duodenale* (known also as the *Dochmus Duodenalis*). The parasite was discovered as early as 1831 by Dubini, and Griesinger twenty years later found it in a patient dead of *chlorose égyptienne*. It was, however, at the piercing of the St Gothard tunnel that the relation between the anæmia and the parasite was clearly established by two Italians, Perroncito and Corseoli. The persons affected presented the various symptoms of pernicious anæmia: pallor of the face and mucous membranes, great muscular feebleness, disinclination and inability for work, headache, vertigo, circulatory and digestive disturbances; the coloured corpuscles presented alterations of form, of number, and of volume; the white corpuscles were present in normal number. These symptoms presented themselves with very variable degrees of intensity in the different cases, but what chiefly characterized the affection was the presence of very severe abdominal pains, of which the patients complained constantly. The number of eggs in the stools was in proportion to the severity of the symptoms. The eggs are oval, formed of a thick membrane, with a double contour, and containing the yolk.

These eggs are produced by the larvæ, which multiply in the duodenum and produce the anæmic state. The treatment consists in administering the ethereal extract of male fern, in ten gramme doses, at intervals of several days, until the eggs disappear from the stools, and then administering ferruginous preparations.—*Le Progrès Médical*, 6th February 1886.

ACUTE RHEUMATISM AND NERVOUS AFFECTIONS.—Brieger calls attention to three interesting cases of acute rheumatism occurring in Leyden's clinique. In the first case chorea was developed in the course of the disease; in the second, sciatica with inflammation of some vertebral joints appeared during the primary affection; and in the third there was sciatica, and at the same time inflammation of the maxillary joint.—*Berliner klinische Wochenschrift*, 8th March 1886.

LOCOMOTOR ATAXY WITH RETENTION OF PATELLAR REFLEX.—Hirt publishes notes of three cases of ataxy in which there was no change in the knee phenomenon. Two of these were syphilitic, and the third patient, a woman, probably had the same taint, as most of her children died soon after birth. He states that Westphal has some cases of a similar kind which are to be published shortly.—*Berliner klinische Wochenschrift*, 8th March 1886.

TENDER POINTS IN CHOREA.—Marie draws attention to the fact that in the true chorea of Sydenham tender points have been described over the spine, the thorax, and the abdomen. In addition to these, however, he has noted a number of cases in which there was tenderness over the ovary also, and that this is present on the same side as that on which choreic movements first appeared.—*Le Progrès Médical*, 16th January 1886.

PROGRESSIVE MUSCULAR ATROPHY BEGINNING WITH THE LOWER EXTREMITIES.—Charcot and Marie describe a particular form of progressive muscular atrophy, which begins by affecting the feet and legs without any change in the hands and arms until after the lapse of some years. In this special form there is a relative integrity of the muscles of the root of the limbs, or at least they retain their normal condition longer than those of the extremities. The muscles of the trunk, shoulders, and face remain intact. In the muscles becoming atrophic there are fibrillary contractions, and there are vasomotor troubles in the segments of the limbs affected. There are no noteworthy tendinous contractions of the joints whose muscles are atrophied. The sensibility is usually unaltered, but cramps are frequent. There is reaction of degeneration in the atrophic muscles. The affection commonly begins in youth, often amongst brothers and sisters, and is sometimes hereditary.—*Revue de Médecine*, February 1886.

THE ESTIMATION OF ALBUMEN BY MEANS OF ESBACH'S ALBUMINIMETER.—Guttmann gives the result of his investigations into the practical value of Esbach's method of quantitative estimation of

albumen, a method which has been described in the *Lancet* and the *British Medical Journal*. By comparing the results obtained by means of this method with those found by weighing, he found that of three experiments the first had a difference of .007 per cent., the second of .05 per cent., and the third of .024 per cent. between the results. He warmly recommends the method as being almost exact in its results.—*Berliner klinische Wochenschrift*, 22nd February 1886.

SURGICAL PERISCOPE.

By A. G. MILLER, F.R.C.S.E.

EXCISION OF THE RECTUM FOR CANCER.—Professor Macleod of Glasgow considers that this operation may be very useful in a limited number of well selected cases. He has operated seven times, and concludes from these cases that life may be prolonged and suffering much alleviated. He considers that hæmorrhage need not be feared, provided the knife is used only for the skin incisions, and the finger, with the occasional snip of a pair of blunt-pointed scissors, employed to separate the rectum from its surroundings. To remove the portion of bowel, he recommends splitting it with the thermal cautery, and separating it with the cautery or *ecraseur*.—*Glasgow Medical Journal*, November 1885.

THE RESULTS OF LARYNGEAL SURGERY.—"The operation of excision of the larynx was, we believe, first performed by P. H. Watson in 1866. He was followed by Billroth in 1873, and later by a number of surgeons, including Heine and Gussenbauer, of Prague; Langenbeck, Caselli, of Milan; Schmidt, of Frankfort; Foulis, of Glasgow, and others. In 1884 Hahn (*Arch. f. klin. Chir.*, xxxi. p. 171) collected the statistics of 52 operations for total excision. Out of 22 patients operated upon by Billroth, mostly for carcinoma laryngis, 19 are dead, and 2 more are extremely liable to a relapse. Billroth's cases are much worse than those collected by Foulis. Of 26 cases of total extirpation, 14 died in consequence of the operation. Of the remaining 12, 3 died in relapse. Still more favourable are the results obtained by Professor Thiersch, of Leipzig, reported by Dr A. Landerer (*Deutsche-Zeitschr. f. Chir.*, xvi. 1 u. 2, p. 149, 1882). Of 4 cases of total extirpation for carcinoma laryngis, 1 died in consequence of the operation, 1 died of a relapse three months later, and 2 recovered and had no relapse (seventeen to eighteen months)."—*New York Medical Record*, 19th December 1885.

TREATMENT OF CARBUNCLE.—At a meeting of the Philadelphia Academy of Surgery, Dr James Collins stated that he had for some time treated carbuncle by free incision, scraping out and washing out with corrosive lotion. Patients have done better under this than any other method. Dr S. W. Gross described

the procedure as "far the best operation yet suggested."—*Philadelphia Medical Times*, 31st October 1885.

POTT'S CURVATURE.—Mr William A. Elliot, F.R.C.S.I., and Surgeon to the Whitworth Hospital, Drumcondra, records his experience, extending over fifteen years, of a method of treating psoas abscess which is very simple, and, according to the author, invariably efficacious. He draws down the skin, makes a puncture with a lancet, presses out the pus and curdy matter, then allows the skin to return to its original position, thus making the opening valvular, and excluding all chance of air entering. In the present day of elaborate antiseptic precautions and dressings it is interesting and remarkable to find that Dr Elliot, having put on a pad of lint and a bandage, removes the same after three or four days, and has "always found the wound healed." Dr Elliot says "these constitute the only means which for many years I have adopted when performing this operation, and having found by experience that it is perfectly safe and the results satisfactory, I have not found it necessary to make any departure from it."—*The Dublin Journal of Medical Science*, 1st January 1886.

PREDISPOSING CAUSES OF ANEURISM.—Dr John B. Hamilton, Surgeon-General, U.S. Navy, etc., in a paper, showing an immense amount of research into the literature of the subject, and published in the *American Journal of Medical Sciences* for October 1885, comes to the following conclusions:—"Neither syphilis, nor alcoholism, nor occupation, nor heat alone, appears to have any appreciable influence on the causation of this disease (page 400). Syphilis he seems to throw over on the authority of Mr Barwell (*International Encyclopædia of Surgery*, vol. iii. p. 379), and quotes him as attributing aneurism among British soldiers to "tight uniforms, especially the stock." This latter theory he also rejects, because women are not specially liable to aneurism, though they habitually wear stays, and generally tight ones. Alcoholism he rejects as a predisposing cause, having received written statements from the superintendents of several inebriates' houses, etc., to the effect that aneurism was not a cause of death among their inmates, and two of these correspondents say that they have not met with a single case of aneurism among upwards of 10,000 patients. Dr Hamilton says that he found no excessive amount of aneurism among men exposed to excessive heat, such as stokers, furnace men, etc., their mortality was mostly from chest diseases. Climate he considers "the only constant element among all the alleged causes of aneurism." He gives the preference to "warm climates, with great diurnal changes, and humid air" (page 393). As examples he notes the coasts of the more southern states of America, the British Isles, and New Zealand. We do not profess to agree with Dr Hamilton, but simply give his opinions. The numerous tables in his paper are interesting, and show much painstaking research.

Part Fifth.

MEDICAL NEWS.



BURROUGHS, WELLCOME, & Co.'s HYPODERMIC POCKET CASE.—A cheap, neat, complete, and most practically useful little box.

OBITUARY.

THE LATE DR ANGUS MACDONALD.

ALREADY one has become familiar with the fact that Angus Macdonald has passed away. The feelings of shock, of acute sorrow, and of painful regret are blunted, and one can contemplate the loss more calmly than was possible a month ago. The facts of his short life must now be gathered together by a friendly hand, and recorded, both as a humble tribute to his memory and as a lesson of wide and varied application to his contemporaries and to those who will come after him.

Angus Macdonald was born in Aberdeen on 18th April 1836, and he died on the 10th of February 1886, being thus in his fiftieth year. His father, James Macdonald, was a road contractor, and resided in Aberdeen. The first ten years of Angus Macdonald's life were spent in that city, and on his father's death in 1846, his mother, with a young family, removed to the parish of Grange in Banffshire. Mrs Macdonald was a woman of character and of vigorous intellect; she trained her family well, and she lived to see

her sons achieve honourable success in life. Up to the age of seventeen Macdonald lived the life of a vigorous country lad, full of physical energy, which found free play in the sports of youth, and in the sterner occupation of agricultural labour. He worked for several years with his own hands; no one could then have predicted that this son of toil would in after years become a distinguished physician. His modesty seldom allowed him to refer to the struggles of his early years, and comparatively few, even of his personal friends, were aware of them. Laudable ambition, however, stirred him to aim at higher things, and there arose the craving for knowledge and intellectual distinction. There is reason to believe that in early years he was to a great extent self-taught, and for about two years only he attended the parish school, then taught by the late Arthur Gerrard,—a type of the high-class teachers of parish schools so well known in the north-eastern districts of Scotland. Macdonald had a great regard for this estimable man. Under his care he rapidly acquired a good grounding in Latin, in addition to the ordinary branches of education, and in 1855, when he was nineteen years of age, he went to King's College, Old Aberdeen, and obtained a bursary by public competition. This was before the date of the fusion of Marischal College and King's College into the University of Aberdeen, and the standard of excellence implied in taking a bursary in King's College was accounted high, more especially as to Latin. The great test was the "version," or the translation of English into Latin prose; and so high was the standard, that a few serious blunders inevitably shut a man out of the bursary list. One of his competitors on that occasion can recall his appearance vividly, and especially was he struck with the laborious care and the evident struggle after perfection of the lad from Keith, whose general appearance and even his hands showed that not long before he had had very different work to do. For four years Macdonald's life was that of a student at the northern University—hard work during the winter months, daily trudges along the Spittal to the College with the old gray crown, and almost within the sound of the sea, the emulations of college life, and then home for the summer and autumn months. Like many other students his time was, from the very first year, not entirely his own. The means afforded by the bursary had to be eked out by small fees from private teaching, and Macdonald, during the greater part of his University curriculum, taught in the Gymnasium, or privately, for four hours daily. Still, he took a high place in his classes, and when he graduated as M.A. in 1859, he was awarded the Hutton Prize for general excellence, or, in other words, taking him all round, he was the best man of his year. Macdonald often referred to this part of his life, and expressed his indebtedness to some of his teachers, notably to the late Professor David Thomson, who held the Chair of Natural Philosophy. He always had a warm

heart for the northern University, which gave him, as it has given to thousands, a good all-round training, fitting a man for the duties of life.

Having the intention of becoming a minister of the United Presbyterian Church, he attended the Theological classes in the Divinity Hall of that body in Edinburgh for one session; but he soon found that this was not his vocation, and he began the study of medicine at the University of Edinburgh in 1860, when he was twenty-four years of age, considerably older than the majority of students at that stage of study. There is no reason to believe that Macdonald abandoned the ministerial career on account of any special difficulties in the way of theological belief; he no doubt took this step because he discovered that he had not those oratorical gifts so necessary to a preacher. Throughout life he was a humble and devout believer in Christian truth, although no one knew better nor appreciated more correctly the doubts and difficulties that occur to every thoughtful man.

As a student of medicine he soon took a distinguished position in his classes, although he was much engaged in private teaching. A quarter of a century has passed since then, but the writer can vividly recall Macdonald's appearance, more especially in the anatomical class-room. The large frame—the pale, intellectual, full-bearded face—the piercing and yet kindly eye, intent on work—writing with pen and ink in a large note-book, with nervous rapidity, characters illegible almost to any one but himself—seldom looking up except when the great Goodsir uttered a sentence more profound than usual, or insisted on every one feeling the same enthusiasm as himself over a bony protuberance or a foramen “of physiological significance.” One is apt, perhaps unfairly, to think that there were giants in those days; no one, probably, estimates contemporaries truly, and it is well that the imagination of youth sheds a halo of glory around its revered teachers. Be that as it may, no one could pass unimpressed through the influences of that time. Goodsir, Syme, Christison, Simpson, Miller, Bennett, were all men singularly capable of stimulating the intellect and firing the enthusiasm of students. Macdonald, like many others, felt this, and owed much to it, and it was one of his delights in friendly converse to go back on these days. The writer became specially intimate with him about this period, and now his memory brings up the recollection of long walks on Saturdays, when the time was spent in discussing the work of the week, in criticisms of professors, and sometimes in exuberant physical exercise. Macdonald threw energy into whatever he did; and when out in the country, or on Arthur's Seat, running, leaping, or a snow-ball fight cleared away the vapours and braced nerve and muscle for future work. These memories become more precious as they become older.

As already stated, Macdonald at this period did a large amount

of work in the way of private teaching,—in the first years of his curriculum the ordinary branches of general education, and in the later years several of the subjects of medical study. It is necessary to mention this to form a true idea of his life at this time; but it must not be supposed that he did more than stimulate his pupils to work, and ascertained that they mastered the subjects of study. He was a tutor in the best sense of the word; he had immense influence over his pupils; they respected not merely his superior knowledge, but his sagacity and candour; he told them their faults, and rated them strongly; and yet it is characteristic of the relationship between them, that many became his life-long friends. Such work is as far removed from that of the professional coach as can well be conceived, and it is not too much to say that in these days of enormous classes and a crowded curriculum, such work as Macdonald did is to be highly commended, and has almost become a necessity.

Macdonald graduated in 1864, and immediately settled in practice in Edinburgh. He soon became a lecturer in the Medical School, first on *Materia Medica* and afterwards on Midwifery. The first topic he chose not from any special love of the subject, but because a lectureship was a step in professional advancement. His thorough and systematic habits of study soon gave him an extensive knowledge of *Materia Medica*, which he showed in revising, in 1875, Dr Scoresby-Jackson's text-book on that subject. Practice flowed in upon him more rapidly than is the general experience; but from an early period of his professional life he began to specialize in the direction of Obstetrics and Gynæcology, to which, as is well known, he devoted his later years. About this period he became a Fellow of the Royal College of Surgeons, of the Royal College of Physicians, and of the Royal Society of Edinburgh. In 1866 he married a daughter of the late Rev. Dr Finlayson of the Rose Street United Presbyterian Church, and he had thus the comforts and solace of domestic life, and the help of one singularly well fitted to aid him in his busy duties. Years passed on with increasing responsibilities. He became Physician and Clinical Lecturer on Diseases of Women in the Royal Infirmary, and Physician to the Royal Maternity Hospital. No one led a busier life than he did; but in the midst of it all he found time for study, and he made many contributions to the medical societies of Edinburgh and to medical literature. It was characteristic of him that he ever strove to keep abreast of medical science, especially in the department bearing on practice, and this effort cost him many an hour of hard labour when he ought to have been resting. For example, not many years ago he endeavoured to instruct himself in the method of modern histology, and spent the early hours of the morning in this kind of work. This was very like him: what others did he also attempted; there was always in him the tendency to self-improvement, which was one of the strongest motive powers of his life.

During the past years Macdonald gradually changed the character of his practice, and devoted more and more time to Obstetrics and Gynæcology. As a lecturer on Midwifery for a good many years, his pupils were scattered over the country; he formed a connexion with the profession, and his advice and assistance were sought by his medical brethren, with whom he always maintained the most friendly relations. He was also rapidly coming to the front as a practical gynæcologist, and undertook serious operations, such as the removal of ovarian cysts, fibroid tumours of the uterus, operation for extrauterine pregnancy, and others, the records of which are in the Proceedings of the Obstetrical Society of Edinburgh. It is not the purpose of the writer to analyze or criticize these results, even were he capable of doing so, which he is not; but he alludes to them to indicate the kind of work which occupied much of Dr Macdonald's time and attention in later years. Not a few friends feared that this kind of labour was too arduous for him, and that it might have been better had he left it to others; but he took a delight in it, and had he lived there is little doubt he would have achieved distinction in this class of practice, as he did in everything to which he put his hand. As it was, some of the operations were of a brilliant and daring character, and were followed by a fair measure of success.

The work which will probably in after years be regarded as Angus Macdonald's most important contribution to medicine is his volume on the bearings of chronic disease of the heart upon pregnancy, parturition, and childbed, with papers on puerperal pleuro-pneumonia and eclampsia, published in 1878. This volume has been widely read, and is regarded by authorities, both at home and abroad, as a carefully written, lucid, and suggestive work on the subject.

But time and tear and wear began to tell. About four years ago Dr Macdonald caught a cold, which ended in an attack of pleuro-pneumonia. This necessitated an entire abandonment of work for a year,—a serious check to a hitherto prosperous career. He spent several months in the Riviera,—weary months of alternating despondency and hope. On his return to practice, he took more care of his health than he had hitherto done; worked steadily, but not so laboriously; modified his practice more and more in the direction of consultation work; and non-professional friends thought that his health had been restored. But he himself, and, at least, one medical friend knew better, and they always dreaded that the mischief in the lung would again threaten life. This it did early in the present year; he was prostrated by another pulmonary attack, and he died on the 10th of February—meeting the end with becoming fortitude. Near the close his clear intelligence grasped fully the facts of his own case, and he calmly discussed them with the medical friends who stood around his bedside. It is also interesting to know that, even in the last extremity, he was able to testify his faith in the following

memorable words :—"I have always found the truths of evangelical religion give entire satisfaction to my whole moral nature." A few days afterwards he was laid to his rest in the Grange Cemetery. He left a widow, two daughters, and five sons to mourn his loss.

Such is a brief and imperfect record of the life of Angus Macdonald. His restless energy enabled him to do more work in his time than most men ; and one cannot help feeling that it might have been wiser to have attempted less and to have lived longer. But a life of inglorious ease would not suit him ; he rejoiced in his own intellectual powers, and there was a feeling of masterfulness about him that prompted him to ever-renewed exertion. He was a courageous soul ; and whilst not afraid of looking difficulties in the face, he was filled with the stern resolve to overcome them. His intellect was keen and piercing ; few could more easily brush aside sophistry and get to the marrow of a question ; his judgment was sound and reliable. These qualities gave his advice great value : they were qualities so apparent to all that he quickly won respect and confidence. But it must not be supposed that in his nature the intellectual qualities were predominant. Along with these there was deep kindliness of nature, a sympathy with suffering and distress not ostentatiously expressed by words, but manifested more by a tenderness of eye and a friendly pressure of the hand. His kindness took many a time a practical turn, and the widow and the orphan have cause to revere his memory. He was a true friend, deeply interested in the success of those of his circle, ever ready to encourage and to help. Only those who have had experience of it can understand the charm of his personal friendship.

Taking him all over, Angus Macdonald was a noble type of man. A faithful worker in his day, he has also, for his personal qualities, left a memory that will long be cherished by those who had his friendship. In the afternoon of life, as one sees by the eye of the imagination the procession of those who have gone before, there is no shade that is more welcome than his, as it reminds us of a life well spent, of duty nobly done, of a great example of what can be accomplished in the way of good work in a high sphere of usefulness, even by one whose path at the outset was surrounded by difficulties of no ordinary kind.

The following remarks in reference to the professional career of Dr Macdonald were made at the meeting of the Edinburgh Obstetrical Society, on 24th February, by the President, Dr Halliday Croom :—

I could have wished that the sad task of speaking of him whose death to us all here is an irreparable loss had fallen to one of you able, I will not say better to appreciate, but certainly more fitly to express the highest esteem and honour in which Dr Angus Macdonald was held by us.

As through your kindness for the time being President of the

Obstetrical Society, it is my duty to place in your name "a minute with regard to the late Dr Angus Macdonald," and it is to me a matter of no little solemnity that I should be called to speak of him to you in these circumstances. His name is associated in my mind with memories entirely personal, and which, even could I express in words, I could scarcely expect any of you fully to appreciate; and for this reason, that the family of which through marriage he became a member is associated in my mind with all thoughts of old home days. Indeed, I believe that a somewhat similar task fell to my father in a former generation which falls to me now. I perform it with the same diffidence, and as myself being compelled thus sternly to realize the flight of time, I hope with the same solemnity.

Here and now we may not speak of Dr Macdonald in any relation than that which he bore to the profession, of which he was a most distinguished member, and to this Society, which was so honoured by having his name on the roll-list of its active members and former Presidents.

What he was to his family they alone know. The irreparable loss—the much he might have been to them—they can but the more keenly know as the years roll on. With them I will ask you, through our Secretaries, to express our deepest sympathy and distress. Into their sorrow we may not intrude.

"The heart knoweth its own bitterness," and

"By the hearth the children sit, . . .
They see the vacant chair, and think
How good, how kind, and he is gone!"

You will allow me, in those classic words, to express for his widow what I am sure is the wish of you all:—

"May all love—his love unseen, but felt—o'ershadow thee;
The love of all thy sons encompass thee;
The love of all thy daughters cherish thee,
Till God's love set thee at his side again."

What the famous Berlioz said of his Leipsic physician we may more truly say of Dr Macdonald as a practitioner and a colleague, "*Inter doctos clarus sed patientibus carus.*" To his patients he was universally kind, considerate, and attentive—interesting himself, I have the best reason to believe, personally, permanently, and practically, in each case. Many will miss in him one who lightened their suffering not more by his skill than his friendly kindness. Among the members of the profession he was essentially *clarus*, as, indeed, he was in his whole career. I need not call to mind the prominent place he took in a sister University and a sister branch of science amid many difficulties, which to other less morally robust natures would have been insuperable. His extensive general knowledge, his wide sympathies and strong convictions, were built on a basis of broad culture falling to the lot of few in our profession. Thus was he in judgment without that narrowness and one-

sidedness which is too apt to result from a devotion, however laudable and even necessary, to one branch of study.

As an obstetric author his reputation will rest mainly on his important published study *On the Relation of Chronic Heart Disease to Pregnancy and Parturition*. Originally communicated in separate papers to the Society, it is a typical example of the hard, heavy, thorough work Dr Macdonald was able to do; and as a Society of Edinburgh men we have good reason to be proud of it. I need scarcely stop here to enumerate the various contributions he made to this Society. They were as numerous as they were varied, and are fresh in the minds of most of us. They were one and all the result of patient, thorough research,—of that keen, critical, and analytic faculty which in so large a measure was characteristic of Dr Macdonald's mind.

As a consultant in obstetric cases, he enjoyed in an eminent degree the confidence of his professional brethren, for he brought to them a mind mature in thought and rich in experience. How much his opinion was valued and relied on will be felt keenly by those men wont to seek his willing and able help in all emergencies.

As a gynecologist, while he was pre-eminent in every department, perhaps for us here the most striking feature in his career, and one full of lessons and encouragement for most of us, was the extraordinary perseverance he showed in training himself as an operator. Recognising, as he did, on his appointment to the Infirmary the place which abdominal section was destined to occupy in gynecology, he formed the laudable ambition to excel in this work. Comparatively untrained in surgery, with at first somewhat awkward methods and little resource, he doggedly applied himself to master the details of the work, and he did it with marked success. In his later years he showed himself an expert operator, undertaking many of the most grave and difficult cases with brilliant success. To those of us, like myself, who had seen him at the commencement and at the end of his operative work, nothing could be more wonderful than the large and steady development of his facility in operation, and the pluck and determination which he showed in dealing with the most severe and unexpectedly complicated operations. The records of this Society, and of the *Edinburgh Medical Journal*, bear ample testimony not only to his ability but to his continuous success. It is not for me to refer to his statistics. They are not in my possession. This I do know, that when they are published they will show a record of which our Society and our School may well be proud.

Many students will look back with gratitude to the impulse they received from him as a teacher; nor will they forget the lessons of thorough work and accurate diagnosis which he imparted. They will never cease to be grateful for the constant trouble he took with them, and the many acts of unostentatious kindness they received at his hands.

By his death we mourn the loss of a large minded citizen, an earnest teacher, a distinguished physician. Students, professional brethren, and patients, will feel it alike, but nowhere will it be more or more really felt than in this Society.

For years, by constant attendance, active work, and heartfelt sympathy, Dr Macdonald has been a pillar to our Society. Successively Vice-President and President, he has adorned its highest offices. His great name, the basis of long and wide experience, the penetrating and unerring insight on which it rested, have added at once to its reputation and to the permanent and grateful gain of its Fellows.

We shall miss at our table his grave earnest face, his shrewd sober counsel, his thoughtful and kindly criticism, while our Transactions will be the poorer that his able pen no longer contributes to them.

It is for you and for me, gentlemen, for what of our lives yet remain, to take to heart the lessons of his laborious and well spent life. We may receive from it a fresh stimulus by seeing what conquests may be made through steady, enduring, and unremitting toil. For him there was no day without its line, none in which he did not acquire or try to acquire some new fact or some new light; and thus it was, through hard work and devotion to duty, which are three parts of genius, he rose surely from comparative obscurity, pushed his way through obstacles which only his strong moral courage and indomitable perseverance could have surmounted, and reached that most eminent position amongst his professional brethren and amongst the general public which he occupied at the time of his death.

Of his religious convictions, which were both strong and deep, it is no part of my duty here to speak, except thus far, that they were manifested—and how better could they show their intense reality?—in stern devotion to the work of his life. They enabled him, though cut off in the midst of his days, to finish—no doubt imperfectly, as everything human must be imperfect, but as only very few can claim to have finished—the work given him to do.

JAMES ARCHIBALD SIDEY, M.D., L.R.C.S. Ed.

It is with much regret that we have this month to record the death of a well-known and much respected Edinburgh medical practitioner, Dr James A. Sidey. Some time ago, and before any symptoms of an immediately alarming nature occurred, Dr Sidey seemed to have suffered from overwork and what was considered to be a severe attack of cold. A day or two previous to his death, however, matters assumed a more serious turn, and terminated somewhat suddenly in the fatal result which took place at his own residence in Heriot Row on the morning of February 24th. Dr Sidey was the son of a medical man of considerable standing in Edinburgh, and to whose very large

practice he in a great measure succeeded. He was born in the year 1825, and was consequently aged 61 at the time of his death. Dr Sidney leaves a widow, three daughters, and four sons, one of the latter being a medical practitioner in Edinburgh. No man was more arduous or indefatigable in the pursuit of his professional duties, in which he was looked up to with the confidence, affection, and respect of a large circle of patients, by whom his loss is now sincerely regretted. But while diligent in the pursuit of his professional calling, he found time for the exercise of various natural gifts, of which he was possessed. Of an inquiring and reflective turn of mind, he had collected a vast amount of facts and observations on matters which generally escape the notice of most persons, and it was a pleasure to him to draw the attention of his friends to the new phase in which some simple occurrence would present itself to his view. He was, besides, fond of collecting objects of artistic or literary or scientific interest, and the attractions which his house presented in this way were well known to all his acquaintances. Dr Sidney was the author of many songs and verses, most of them of a humorous nature, and sparkling with fun and vivacity, while not a few display an equal amount of tenderness and pathos. These were nearly all published some years ago in two consecutive volumes, the *Mistura Curiosa*, and *Alter Ejusdem*—while several of them, along with others, are to be included in a collection of similar compositions now in course of publication. The doctor was naturally of a modest and retiring disposition; he did not mix to a very great extent among his professional brethren, nor did he take any active part in their discussions, either upon matters of practice, or in the controversies arising upon medical legislation. He was, however, a shrewd and successful practitioner, and enjoyed the confidence of some of the leading surgeons of the day. He at the time of his death held the office of surgeon to the jail, an appointment to which he had been elected many years ago, and in which he took much interest. At a former period he had also acted as Secretary to the Obstetrical Society. Altogether he was one of the remaining few practitioners who found time and opportunity, amidst the cares and anxieties of an able professional life, for genial intercourse among his fellow-mortals. His curling and his angling songs show their author's admiration of the roaring game and of the gentle art.—Peace be with him.

JOHN BISHOP, M.D.

AGAIN we have to chronicle the loss by death of one of our Edinburgh *confrères*—a man of great ability, whose short career was full of promise. John Bishop was born in Sheffield; he was educated there, and for some years engaged in mercantile pursuits; but these were not congenial to him, his sympathies were with science, literature, and philosophy, and he felt that in the study of medicine he would find a more suitable sphere. Accordingly,

in 1866 he enrolled himself as a student in Edinburgh University. As an undergraduate he had a distinguished career.

In 1870 he was admitted M.R.C.S. Eng., L.R.C.P. Ed., and in the same year graduated as M.B., C.M., of Edinburgh University.

Immediately after taking his degree he was appointed resident surgeon in the clinical wards of the Edinburgh Royal Infirmary by Prof. Lister, and he was afterwards resident physician under Dr Grainger Stewart. When he left the Infirmary Sir Joseph Lister asked him to act as his private assistant, which office he held till Mr Lister removed to London. At the same time he commenced private practice, devoting his attention more particularly to surgery. During his student days he was an active member of the Medical Students' Christian Association; he was much interested in medical missions, and soon after commencing practice he joined the staff of the Edinburgh Medical Missionary Dispensary. He was also a director of the Society, and when it had been decided to raise funds for a memorial to David Livingstone by means of a bazaar, he became a member of the committee—indeed, no member of it was more active or more interested than he.

On graduating as M.D., he was awarded a gold medal for his thesis on "Antiseptic Surgery." When the Scottish Metropolitan Assurance Company was established, Professor M'Kendrick and he were appointed medical referees to the Company, and later he received the appointment of Assistant-Surgeon to the Edinburgh Royal Infirmary.

About four years ago, while performing the duties of this office, he was infected with erysipelas. The illness was a severe one, and several months elapsed before he felt that he had recovered his wonted vigour. The following summer he became anæmic, and went to the south of England, hoping to return after his usual holiday with renewed energy, but from that time his health deteriorated, and although many changes of climate were tried, his condition gradually grew worse, and he died at Cannes on Saturday the 6th March. During the Christmas holidays Sir J. Lister went expressly to Cannes to consult with Dr Frank as to his condition—a striking evidence of the affectionate regard entertained for him by his former master.

Dr Bishop was a man of noble nature, with wide sympathies; his best qualities did not lie on the surface; he was a loyal and interested friend. Those who knew him best prized him most, and many of his former patients and professional friends will miss his sympathy and counsel. Not only was he possessed of undoubted ability, but all his work was marked by thoroughness and conscientiousness. Whatever he undertook he endeavoured to perform as if success depended upon his exertions. He was of nervous temperament, and the anxieties of life—especially those incidental to the practice of his profession—told seriously upon him.

About six years ago he married Miss Bird, the well-known authoress of books of travel, who survives him.

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F. J. B. Quinlan, M.D., L.R.C.S.I., F.K.Q.C.P., M.R.I.A.,
etc., etc., *Professor of Materia Medica and Therapeutics, Catholic University Medical College; and Examiner in Materia Medica and Therapeutics, Royal University, writes in the "Medical Press," July 15th, 1885.*

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Part First.

ORIGINAL COMMUNICATIONS.

I.—ON THE ETIOLOGY OF VAGINAL HÆMATOMA OCCURRING DURING LABOUR.

By J. HALLIDAY CROOM, M.D., F.R.C.P.E., F.R.S.E., Physician to the Royal Maternity Hospital; Physician for Diseases of Women, Royal Infirmary; Lecturer on Midwifery and Diseases of Women, Edinburgh.

(Read before the Edinburgh Obstetrical Society, 13th March 1886.)

SANGUINEOUS tumours, thrombi and hæmatoma of the vulva and vagina, whether occurring before, during, or after labour, are comparatively rare accidents. Under such a heading are included all collections of blood forming in the submucous or connective tissue, and they may be found in the cervix, any part of the vagina, labia, or perimetric connective tissue. The condition receives more or less attention in every text-book, and the varieties, with their progress, terminations, and treatment, have been exhaustively discussed, in lengthened memoirs, by Deneux, Laborie, Cazeaux, Hervieux, Hugenberger, and Barnes, and have received shorter notice from many others. Into the general question of the nature and management of these hæmorrhages in general it is no part of my present paper to enter. I wish, therefore, to eliminate, first, the blood extravasations which are limited to the cervix, and which in a minor form are familiar to most of us. In an aggravated and serious form they are at times associated with elongation and hypertrophy. This much I wish to point out, that they are the result of direct pressure and crushing. Second, Those which form in the labia vulvæ, and which are the result of unusual pressure applied to already distended and varicose vessels.

My remarks apply entirely to that form of hæmatoma which is vaginal, usually confined to the posterior wall, and is developed during and after labour. Three such cases have come under my observation, and I now give a brief record of each.

CASE I. occurred in the hands of one of my dispensary pupils. Mrs M., a multipara in labour with her sixth child. When I saw her the membranes had ruptured for twelve hours, and the head had made no progress though the pains had been strong. On examination I found the head at the brim and movable; os fully dilated. The cause of delay was obvious. The woman's lax abdominal

walls had allowed the uterus to become anteverted,—so much so, that the fundus uteri was on a level lower than the cervix, and consequently the uterine action was quite unavailing so long as the direction of the force was thus misdirected. I ordered the patient to have a tight abdominal bandage applied, and to have a full dose of ergot. Two hours afterwards the child was born. Next day I was asked by my pupil to see the woman, who complained of great bearing down pains, and something protruding at her vulva. On examination I found the perinæum bulging, and a dark swelling protruding from the vagina. I found the whole vagina filled by a large pyriform mass, base downwards, stretching from the posterior fornix down to the perinæum.

She suffered much pain during the night, with strong expulsive efforts; and when she was seen in the morning, a rupture had taken place at the lower end of the vagina, through which had escaped free venous blood and clots. There continued considerable oozing from the aperture for a fortnight, during which time careful antiseptic washing was employed; and at the end of three weeks the tumour had entirely disappeared.

CASE II. occurred in the extern department of the Maternity Hospital. The patient was a multipara in labour with her fourth child. As the patient had been in labour for ten hours, as the membranes had been ruptured for six, and as no progress was made, I was called to see the case and deliver with forceps. I found the pains very violent and strong; the head presenting, but not engaged in the brim. The uterus was markedly anteverted, the patient being a big, heavy woman, with a very pendulous belly. The uterine body was thrown well forwards, and somewhat flexed. The delay was evidently caused by the misdirection of the uterine force. I placed the woman on her back, and had a binder applied.

On introducing my hand, some fifteen or twenty minutes later, into the vagina, I felt a distinct bulging in the fundus vaginæ in the posterior fornix. The nature of this I did not at the time recognise. As the head was movable at the brim, and did not engage even after the position of the uterus was rectified, I performed podalic version. I found a little difficulty in extracting the head from the vagina. With the last pains the placenta and membranes were expelled *en masse*. As violent expulsive efforts continued, and as she complained of great pain, and, although there was no hæmorrhage beyond normal, she looked somewhat collapsed, I made a vaginal examination, and found a large tumour bulging the posterior vaginal wall. It extended in a conical shape, the base being at the vulva and the apex at the cervix. Next morning when I saw the patient I found she had passed a restless night. There was pelvic pain, bearing down expulsive efforts, and retention of urine. On examination the vagina was so occluded that I could not reach the cervix. The conical nature of the tumour, its intimate connexion with the mucous membrane, its firmness and

tenderness, as well as the fact that it reached quite down to and bulged the perinæum, combined with a rectal exploration, decided me in coming to the conclusion that it was a hæmatoma of the vagina. Rest, quiet, and opium were enjoined. The next day she was free from pain. There was no rupture of any part of the tumour. In four or five weeks the patient was well, and no trace of the tumour remained.

CASE III.—Through the kindness of Dr Alex. Sinclair, I saw with him a patient whom he had delivered two or three days previously with forceps. She was a multipara with her fifth child. Dr Sinclair had used forceps because the head was delayed high up, owing to some degree of anteversion of the uterus. The patient had borne her previous children normally. Dr Sinclair told me that when applying his forceps he felt a swelling in the posterior fornix, and on that account experienced some difficulty in applying the instrument.

The swelling on the third day was so large as to fill up the whole vagina, and so closed the canal as to make it impossible to reach the cervix. The perinæum was bulging, and at the junction of the perineal skin with the vaginal mucous membrane there was a rupture which had occurred the previous day, when Dr Sinclair was examining the tumour. The aperture was sufficiently large to admit two fingers, and on doing so the finger came in contact with a mesh-work of cellular tissue and clots. The nature of the tumour was clearly that of a hæmatoma. I need not stop to describe its further course beyond saying, that the cavity was washed out daily with an antiseptic lotion, the patient had no bad symptoms, and in a few weeks was quite well.

The rarity of such cases may be gathered from the following statistics, viz.,—Hugenberger, St Petersburg Hospital, met 11 in 14,000 labours. Deneux saw 3 in forty years. Paul Dubois 1 in 14,000 labours. In Vienna Hospital 18 occurred in 33,241 deliveries.

The records show that these extravasations are associated with very considerable danger. Deneux, out of his 60 cases, had a mortality of 11. Probably it is owing to this fact, Deneux being the earliest writer on the subject, that the opinion has gained firm ground that this condition is an extremely dangerous one. Now, this is scarcely the fact, for out of 50 cases collected by Winckel, death occurred only six times; and the three cases which I had just mentioned all recovered perfectly, and one had a normal labour subsequently. No doubt a good deal depends on their extent and situation, but so long as they are confined either to the vulva or vagina, remain of moderate dimensions, and do not begin in or extend to the perimetric tissue, they seem to be less serious than was at one time supposed. My present object is to point out what I believe to be the etiology of this form of vaginal hæmatoma, or, at all events, *one* of the causes. In whatever respects the cases I have recorded may differ, they had the following points

in common:—*1st*, The women were multiparæ; *2nd*, The labour was delayed; *3rd*, The cause of delay was misdirection of the uterine power from anteversio uteri or pendulous belly; *4th*, At the occurrence of the hæmatoma the head was still at the brim.

First, then, there is the common feature of pendulous belly. This, of course, when not associated with narrow brim, as it sometimes is in primipara, means anteversion of the gravid uterus from relaxation of the abdominal walls.

This is a question of degree so slight that the contraction of the uterus may be sufficient to remedy the displacements, or to such a marked degree that the fundus uteri comes to lie on a level lower than the cervix. Fig. 1 shows the varying degrees of pendulous belly, from slight inclination of the fundus forwards to that where the fundus lies on a level lower than the cervix.

Nay, more, cases have even been related where, owing to a split in the recti muscles, the gravid uterus has prolapsed through the aperture and reached almost to the knees, covered only by skin and fascia. In even moderate cases there is a certain amount of flexion as well, the under part of anterior wall of the uterus being bent over the anterior wall of the pelvis, the symphysis pubis acting as a fixed point (Fig. 2). In this way tension is made to a very marked degree on the vaginal walls. This tension is produced by two factors—*1st*, The tilting forward of the uterus over the symphysis—this, of course, being in direct proportion to the amount of anteversion; *2nd*, By the uterine pains. They will, by drawing on the posterior vaginal wall, increase the tension already caused by the altered position of the uterus. (This condition is shown in Fig. 2.) It will be apparent, and it is a fact borne out by clinical observation, that the symphysis pubis acting as a fixed point, the greatest tension and stretching will be on the posterior vaginal wall. In an interesting and suggestive paper read to this Society some years ago by Dr Hart on Rupture of the Vagina, he drew attention to two facts bearing on the present subject—*1st*, That vaginal rupture is generally high up on the posterior vaginal wall; and *2nd*, When it does occur, it is generally transverse. The posterior vaginal wall, especially at its upper part, is, during pregnancy, very thin. This thinning is increased in a normal labour by the uterine action dragging up the cervix and vaginal walls; but in the cases I have just referred to, there is the very great additional stretching induced by the abnormal position of the uterus, and the long-continued ineffectual contraction of the uterine walls. The venous supply to the vaginal wall consists of a large plexus of veins, some submucous and some just outside the muscular coat, emptying themselves into the internal iliac. These veins, in common with the other veins in the pelvis, are valveless. Fig. 3, after Savage, gives some idea of the number and close distribution of these veins. In pregnancy these veins, to a greater or less extent, become varicose, owing, no doubt,

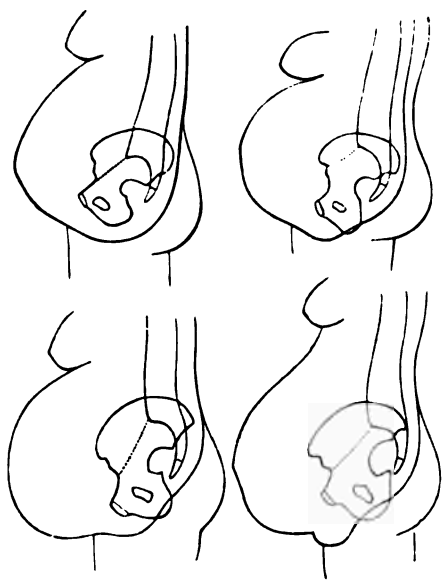


FIG. 1



FIG. 2.



FIG. 3.

to the heavy uterus preventing the entrance of the venous blood to the vena cava. So uniformly is this varicosity present, that, under the name of Jacquemier's test, it has come to be recognised among the corroborative signs of pregnancy. Often the venous tissues become so distended with varicose enlargement that they become quite perceptible to the finger in the vagina, and can be felt projecting as well-marked cyst-like swellings. Further, it must be borne in mind that, in proportion to the amount of varicosity, so will the wall of the vessel be thin. Hervieux, Laborie, and others are not inclined to believe that varix enters much into the production of these vascular swellings. Perret has collected a series of sanguineous tumours of the vagina, cervix, and vulva, and points out that they occur more frequently by five times in primiparæ than multiparæ, and that they become proportionately rarer according to the number of the pregnancy. Now, in this record Perret mixed up all the varieties. One can easily understand how, under the enormous distending pressure on the cervix and vagina caused by the advancing head, the mucous membrane, carried down by the head, glides over the deeper tissues with a sort of tearing movement. The connecting submucous vessels are torn, blood is extravasated, and small ecchymoses or thrombi are formed. This will to a greater or less extent occur in every labour. But we must not be diverted from the point at issue. The cases I have recorded were all multiparæ, and hæmorrhage took place while the head was still at the brim, and while no pressure was being made in the vagina. The mechanism of the production seems to me to be this,—The thin varicose veins on the posterior vaginal wall all undergo stretching along with that structure by the uterine efforts; but, in the cases referred to, the tension is enormously increased by the anteverted position of the uterus; and further, this tension is long maintained by the ineffectual attempts of the uterus to right itself. What occurs is this: during the labour one or more of these vessels give way under the strain, and rupture, but owing to the tension no hæmorrhage takes place. When, however, the case is recognised, and the uterine obliquity overcome, either by position or pressure, or both, the undue tension of the posterior vagina relaxed, the compression of the vessels removed, the hæmorrhage slowly and gradually takes place into the connective tissue. In this way it comes about that the rupture of the vessels takes place during the labour, but that the hæmorrhage actually occurs only after the displacement of the gravid uterus has been rectified. Into the prognosis, course, symptoms, and risks of these varieties of tumours it would be altogether absurd in me to enter; they are well described in every text-book. My object is simply to draw attention to a hitherto undescribed cause of their production, and if I may draw a practical lesson, I would add this as one of the risks of labour complicated with a pendulous belly, and an additional reason for recognising and rectifying this form of dystocia early.

II.—ILLUSTRATIONS OF UNCOMMON MORBID CONDITIONS OF THE EAR, NOSE, AND LARYNX.

By P. M'BRIDE, M.D., F.R.C.P. Ed., F.R.S.E., Surgeon to the Ear and Throat Department of the Edinburgh Royal Infirmary, and to the Edinburgh Ear and Throat Dispensary; Lecturer on Diseases of the Ear and Throat, Surgeons' Hall.

(Read before the Edinburgh Medico-Chirurgical Society, 3rd February 1886.)

IN this paper I propose to bring under the notice of Members of this Society some cases which seem to me of more than ordinary clinical interest—illustrating as they do morbid conditions and symptoms which are less commonly met with, or which are liable to erroneous interpretation.

(1.) *The Prognosis of Chronic Non-suppurative Middle Ear Affections.*

It is foreign to my purpose to attempt here an exhaustive discussion of this theme, to which, indeed, I have elsewhere referred at length.¹ The fact I now wish to emphasize by illustrations is that the surgeon should not be too ready to regard as hopeless cases in which the tuning-fork is heard by bone conduction worse in the deaf or deafer ear, and in which tinnitus is a more or less constantly present symptom. This is demonstrated by the results in the two following cases, the histories of which are necessarily somewhat briefly recorded, owing to the fact that they occurred in private practice:—

Mr —, æt. 50, consulted me first on the 3rd October 1885. He had then suffered from singing in the ears for about 18 months, and slight deafness for a year.

Unfortunately, at this patient's first visit, my own watch was undergoing repairs, but that which I was wearing was heard on the right side at 12, and on the left side at 33 inches. This watch had a very loud tick, and could be easily heard at a distance of 15 feet by a healthy ear. The vibrating tuning-fork, when applied to the middle line of the forehead, was heard better in the left ear. Moreover, while it was perceived longer opposite the meatus than from the mastoid on the right side (*i.e.*, the ear which was more deaf), it was perceived better from the mastoid than opposite the meatus by the left or better ear. In other words, bone conduction was more interfered with on the side corresponding to the ear which was most impaired.

Both drum membranes were slightly thickened, but otherwise normal; air, however, entered the left tympanum more readily than the right by Valsalva's experiment. The patient had had attacks of giddiness, one rather severe, about a year ago. There

¹ *Transactions of the International Congress, London, 1881.*

was a history of naso-pharyngeal catarrh, and posterior rhinoscopy revealed the presence of slight congestion in the posterior nares. Inflation by Politzer's method improved the hearing on the left side, but not on the right. A guarded prognosis was given; the application of iodine, and the inflation of chloride of ammonium vapour by Valsalva's method recommended.

On the 8th of October the patient returned, and stated that he had suffered much less from tinnitus, which had, indeed, been quite relieved until the previous day, when it returned slightly. The tuning-fork was not heard better by bone conduction in the right ear, *i.e.*, the side on which it had previously been less distinctly perceived. The hearing was still far from perfect, but on this occasion Politzer's inflation improved the hearing of both ears. The patient was advised to continue the same treatment for a fortnight, and if not quite well to return. I did not after this see him again until the 24th of December, when I learned that he had considered himself perfectly cured, but had just been attacked again by the tinnitus. This relapse was again accompanied by *impaired bone conduction* on the right side, and, of course, this time, guided by previous experience, I ventured upon a favourable prognosis.

The impairment of hearing in this gentleman, which was present even after all tinnitus had disappeared, and was not sufficient in amount to interfere with his business or social intercourse, although noticeable when tested with the watch, was undoubtedly due to thickening within the tympanum. Every now and then, associated with obstruction, more particularly of the right Eustachian tube, tinnitus, impaired bone conduction, and sometimes vertigo set in. Whether this temporary impairment of function on the part of the auditory nerve, with occasional irritability of the semicircular canals, be due to physical changes in the middle ear, or to secondary hyperæmia of the labyrinth, cannot be positively decided.

The next case which I shall bring under your notice is in many respects similar.

Miss —, a lady of plethoric habit, consulted me on the 25th Sept. 1885, on account of a noise in the left ear which had lasted for a week, and a feeling of giddiness. The throat had been rough for some weeks, and, unfortunately, I have no detailed notes on the results of rhinoscopic examination; but, so far as I remember, there was naso-pharyngeal catarrh, slight in amount. Hearing was represented by 9 inches on the left side, and 20 on the right—the normal distance for my watch being 30 inches. The tuning-fork from the forehead was heard, so far as the patient could judge, only in the right ear.

Objective examination showed the left membrana tympani to be much indrawn, while the right was somewhat thickened but unchanged in position. Passing the Eustachian catheter, and forcing air through it, relieved the tinnitus for a few seconds only.

On the next day the tinnitus was still marked and troublesome, while the Eustachian catheter raised the hearing power to 17 inches on the left side. On the 28th I again saw Miss —, and made the following notes:—H.D. $\frac{10}{30}$. Tinnitus remained away for half an hour after last visit, and has been less until this morning. Valsalva's inflation opens the left Eustachian tube, and raises the hearing to $\frac{14}{30}$, which the catheter further improves to $\frac{17}{30}$. Tinnitus is of two kinds, viz., resembling (1), trickling of water; (2), whistling. The latter can be relieved by patting the temporal region.

The application of a mixture of capsicum and ammonia to the temporal and mastoid regions, the chloride of ammonium inhaler by Valsalva's method, and the internal use of Kissingen and Harrogate waters, were advised. The result of this treatment was so satisfactory that on the 4th of October the hearing power was equal in both ears, and the patient completely relieved of her discomfort. Moreover, the vibrating tuning-fork applied to the forehead was now perceived equally on both sides.

The remarks which I have made on the previous case apply, in most respects, to this one also. Both cases are interesting alike from the specialist's and the physician's point of view, but it is only to the latter that I shall here refer. Both patients presented all the symptoms of Menière's disease, and in both I at first believed that I had to do with middle ear catarrh, associated with organic lesion of the internal ear; yet the alarming symptoms yielded speedily to treatment. *I would, therefore, draw the conclusion, that in every case of suspected Menière's disease it is the duty of the practitioner to make a careful objective examination of the ears; and not only that, but also to try the effects of rational remedies applied to the middle ear whenever it is possible that it may be in a catarrhal condition, and when deafness is not so absolute as to prove disorganization of the peripheral endings of the auditory nerve.* In this connexion it must not be forgotten that chronic non-suppurative otitis media may run its course with but very slight objective changes. I shall now refer to a case which, while illustrating the negative value of the tuning-fork test, also demonstrates the fact that even when ear disease is present, together with tinnitus, it should not be always assumed that the two conditions stand to each other in the relation of cause and effect.

It has long been my belief that in many instances tinnitus aurium is immediately due to changed tension in the blood-vessels of the labyrinth. It is obvious that such a change of tension may be due either to increased or diminished pressure upon the labyrinthine fluid (such as occurs in ear disease), or to changes in the quality of the blood itself (such as may be due to anæmia, gout, etc.) Again, increased or diminished force of the circulation or vaso motor changes may lead to the same result. In fact, I

believe that while the labyrinthine circulation is under normal conditions it is not perceived as sound, but that this occurs with every deviation from the normal. An exact parallel is found in the heart-beat which is not felt in health, but is perceived whenever it is either abnormally strong or weak. How important are attention to the general health, and more especially to the condition of the blood, in treating tinnitus the following case shows:—

Miss —, æt. 42, consulted me on the 11th October 1885, and had then suffered from tinnitus in the left ear for eighteen months. The sound heard by the patient seemed to be hammering and synchronous with the pulse. At first it came and went, but at the time of her visit to me had become constant. The patient had consulted a London aurist of eminence, who gave a very bad prognosis, which, in conjunction with the tinnitus, gave rise to great depression. Hearing distance, L. †† R. normal, while the tuning-fork in this case also was heard by bone conduction better in the right or good ear. The tympanic membranes were normal, and moved freely with Valsalva's inflation, which also improved the hearing power of the left ear to 20 inches. The use of chloride of ammonium inhalations and the application of iodine over the mastoid were advised, with the result that Miss N. returned on the 19th of October with perfect hearing in both ears. The tinnitus, however, persisted, so that some further cause than the slight Eustachian catarrh, which had disappeared, must be sought. Although the patient had a remarkably ruddy complexion, examination of the mucous membranes and auscultation elicited evidence of anæmia. Bland's pills and hydrobromic acid were prescribed, and their use was followed by great improvement, so that on the 2nd of November the tinnitus had almost quite disappeared, and I have since heard that she is now quite well, and once more able to enjoy life.

This paper would exceed its proper limits were I to refer further to the subject of tinnitus generally, and I can only add that, next to local affections of the ear, I believe anæmia and lithæmia are its most common causes. It might be urged that the cure of the tinnitus in this case was due to the hydrobromic acid prescribed, but a somewhat extended trial of this drug has led me to be very sceptical as to its efficacy, and I have no hesitation in ascribing the beneficial result to the iron pills, with which I have before succeeded in producing similar results in properly selected cases.

(2.) *Circumscribed Papilloma-like Growth in the Inter-arytenoid Fold in Phthisis.*

It is now generally accepted by German writers that, to use the words of Gottstein,¹ "a circumscribed prominent thickening in this situation (*i.e.*, the inter-arytenoid fold) must be looked upon as a sure sign of tuberculosis, even when all other evidences of phthisis in

¹ *Diseases of the Larynx*, by Gottstein. Translated and added to by P. M'Bride, p. 219.

the lungs and larynx are absent." Stoerk¹ fully corroborates this view when he writes:—"This condition of the inter-arytenoid fold is only of diagnostic value when all signs of tubercle in the lungs are absent, and even the most skilled physician can find no grounds for suspecting its existence. In such cases I consider the occurrence of such granulation tumours on the posterior wall (of the larynx) as an unfailing sign of developing tuberculosis." Considering these views so definitely expressed, it is rather astonishing how little mention is made of inter-arytenoid tumour as a sign pathognomonic of tuberculosis in English text-books on laryngology.

The following case illustrates this condition:—

A girl, æt. 20, was sent to see me by Dr Morison, of Hartlepool, and examined at the end of September 1885. The history was to the effect that she had lost her voice for twelve months. Laryngoscopic examination showed slight general congestion, and the presence of a flat tumour springing from the inter-arytenoid fold, which, by its projection into the larynx between the vocal processes, prevented their approximation. (Plate, fig. 1.) This tumour was divided into two unequal parts by a sulcus on one side of the middle line. The laryngoscopic appearances led me to further question the patient, with the result of obtaining additional evidence of a tubercular taint. Her sister died of a chest affection; she herself had suffered from cough for eighteen months, had an attack of hæmoptysis at the end of March 1885, and thought she was getting thinner. It may, however, be remarked that at the time of my examination she was well nourished, and even plump. Examination of the chest at this time gave the following results:—(1.) Dulness over both apices. (2.) Expansion fairly good. (3.) Bronchial breathing over right apex. (4.) Coarse crepitations over left apex in front, and friction behind in supra-scapular region. Unfortunately I have not notes of the temperature, and an examination of expectoration for bacilli, kindly undertaken for me by Dr Batten, was negative. Now comes the interesting point in the case. While the cough, aphonia, and even hæmoptysis all occurred before the 3rd of July, a careful examination of the chest was made on that date by Dr Morison, without revealing the presence of any physical signs pointing to phthisis. A laryngoscopic examination, however, on the 11th of July, showed the tumour to be present.² In this case, then, we may assume—as the aphonia had lasted for a year, and the tumour was its only cause—that the growth was present nearly a year prior to the development of the pulmonary signs.

True neoplasms are very rarely, if ever, found growing from the inter-arytenoid fold, so that when a circumscribed swelling occurs in this region we may almost certainly say that it is due to tubercular infection. I say almost advisedly, because it does occasionally happen that a syphilitic ulcer occupies this situation ;

¹ *Klinik der Krankheiten des Kehlkopfes*, pp. 283-4.

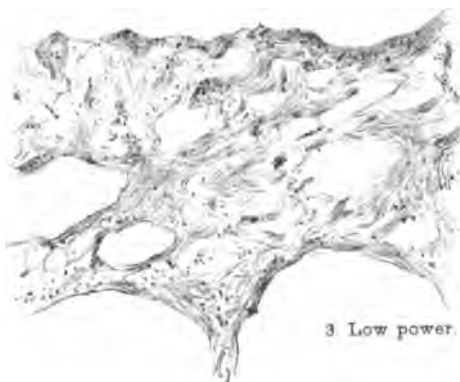
² For these particulars I am much indebted to Dr Morison.



1 (a) Tumour in interarytenoid region



- 2 (a) Cut surface through trabecular structure.
 (b) Sulcus almost dividing tumour into two parts.
 (c) Pedicle



3 Low power.



4 High power.



the upper margins may then grow into papillary excrescences, and by concealing the ulcerated surface lead to a suspicion of latent tubercle. According to Stoerk,¹ phthisical tumours in this region are commonly composed of connective tissue similar to that met with in the walls of phthisical pulmonary cavities.

In a letter, dated 1st Jan. 1886, Dr Morison writes as follows:—"The girl at present is fairly well. The laryngeal growth is certainly less. She continues strong and stout, and her chest about the same, though since coming here she has lost flesh."

(3.) *Erection of the Inferior Turbinated Bodies.*

It is now well known that the mucous membrane of the inferior turbinated bones contains cavernous erectile tissue, more particularly at its anterior and posterior parts. Erection may be due to irritation in some adjacent part of the nose or at a distance. Recently Hack has written at length to prove that various neuroses may be due to increased volume of the inferior turbinated bodies, and he has further succeeded in curing such affections as asthma, megrim, neuralgia, etc., by galvano-caustic destruction of the mucosa in this situation. These views have been more or less corroborated by various writers, and have perhaps led to a somewhat wholesale destruction of nasal mucous membrane with heated platinum points. Still there can be no doubt that many good results have been obtained. Reflex neuroses, which may owe their origin to nasal disease, are thus classified by Schech.²

- | | | |
|---|---|---|
| 1. Neuroses affecting the nose
itself, ... | { | Nervous coryza associated
with paroxysms of sneez-
ing. |
| | | (1.) Ptyalism (rare). |
| | | (2.) Neuralgia, especially
supra-orbital, also me-
grim and occipital
headache. |
| 2. Neuroses affecting adjacent
parts, ... | { | (3.) Vaso-motor disturbance
causing swelling of the
face and conjunctivæ. |
| | | (1.) Possibly vertigo, and epi-
lepsy. |
| | | (2.) Cough. |
| 3. Neuroses affecting more re-
mote parts, ... | { | (3.) Vaso-dilator impressions
to the mucous mem-
brane of the bronchi,
observed by Sommer-
brodt. |
| | | (4.) Asthma. |

In relation to the treatment of the nasal swelling by means of

¹ *Op. cit.*

² *Die Krankheiten der Mundhöhle, des Rachens und der Nase*, p. 193.

the galvanic cautery I believe, from my own experience, that Semon¹ takes up the proper position when he advises that even if no other cause can be found for the neurosis after careful examination, yet no result should ever be promised to the patient from operative interference with the nasal mucous membrane.

The case I am about to refer to was that of a boy 17 years of age, who consulted me on account of his nose being "always stuffed," and headaches—which, on close questioning, seemed to be more of the nature of vertigo than cephalalgia, and which I have little doubt were due to the auditory apparatus, as there was catarrh of both tympanic cavities; I may therefore here state that this case is not brought forward as an example of reflex neurosis. The patient had, however, immense erectile swelling of the inferior turbinated bodies. On the 30th of October anterior rhinoscopy showed this condition most marked on the right side, where the tumour touched the septum. After the application of a 10 per cent. solution of cocaine this diminished to about a third of its original size, and remained small for at least four days. Later, the same treatment was used on the other side, but the effect only lasted for about eight hours. That cocaine has the effect of diminishing erectile swelling of the turbinated bodies has been recognised since the fact was first pointed out by Bosworth; but that its effects in this respect may last for days is not so well known,² and I have thought it worth while to bring forward this example as an illustration.

The further progress of the case, too, was of some interest, for it was found that the probable source of irritation was to be sought in adenoid growths—small in size and amount—occupying the vault of the pharynx. These were crushed, and still further relief was obtained—the other treatment consisting in spraying the nostrils with an alkaline lotion. In view of this action of cocaine, it seems to me that its regular employment should precede operative interference, both with the hope of effecting permanent amelioration of the condition; or if this be on further trial found always to be but temporary, with the view of ascertaining whether keeping under the swelling of the erectile tissue on the inferior turbinated bodies has a favourable effect on a coincident neurosis.

(4.) *Fibro-Mucous Polypus of the Naso-Pharynx.*

According to Mackenzie,³ Panas and other French observers have described tumours growing from the vault of the pharynx, which present a transition of structure from mucous polypi to fibrous tumours. Unfortunately the works of the authorities quoted are not accessible to me in the original, so that I have been unable to

¹ German edition of Morell Mackenzie's work, edited by Semon. Editorial additions, p. 501.

² Dr Cresswell Baker informs me that he has known the effect to continue for ten days. (Communicated by letter.)

³ *Diseases of the Throat and Nose*, vol. ii. p. 532.

obtain particulars as to the exact microscopic structure of these tumours, and I must therefore content myself with giving a brief history of a case upon which I recently operated, together with some pathological details.

Mrs —, a middle-aged lady, suffered for some years from symptoms of nasal obstruction. The history of subjective sensations is somewhat incomplete; but it was particularly noted that the patient complained of nasal obstruction which varied, as she thought, according to the weather, but that there was never any sensation of a movable body in the naso-pharynx. The discomfort seems to have been sometimes very great, obliging her to get out of bed and walk about; at the same time, there was never any attack of an asthmatic nature.

On inspecting the nostrils anteriorly with strong reflected light, a movable mass was seen far back on the right side. Inspection of the pharynx showed nothing abnormal, but posterior rhinoscopy at once revealed the presence of a tumour lying upon the soft palate and filling up the posterior nares. The first view showed the neoplasm as a rounded smooth mass; but after attempts had been made to seize it with forceps, it appeared as if there were two polypi close together.

Digital examination, however, proved that there was only one very mobile tumour. Attempts at removal without anæsthesia were futile; so the patient was put under chloroform, and efforts made to extract the tumour by means of various instruments. The neoplasm was, however, so extremely mobile, that this was found to be impossible. Thus if curved forceps were introduced and the growth guided with the finger, so far as possible, between the blades, their approximation caused it to slide forwards. Again, if a wire snare was passed through the right nostril, it was found impossible to get it over the tumour, because the latter slipped into the posterior part of the left nostril. Dressing forceps, introduced through the nostril, were not used, because it was evident that the pedicle was attached very high up, and could not in this way be reached; and to open the forceps sufficiently to admit the body of the growth without causing serious damage to the osseous structures would have been, I believe, impracticable. The question then arose as to splitting the palate; but being unwilling to proceed to this extremity, I resolved to try what Nature's instrument, in the shape of an index finger, would do, more especially as the mobility of the tumour was such as to lead me to believe that it must be attached by a very long and slender pedicle. I therefore again passed in my finger, and by wedging the tumour against the left Eustachian prominence, succeeded in getting it away.

When first removed, the tumour was between the size of a pigeon's and a hen's egg, but it has now diminished considerably. The pedicle was about $1\frac{1}{2}$ inch in length, and the growth itself is

divided into two parts by a deep sulcus, almost as if there had originally been two tumours, although, of course, the single pedicle negatives such a possibility (see Plate, fig. 2). The shape, I take it, was produced by the growth being constantly forced against the posterior margin of the septum narium by the respiratory current. My friend Dr Alexander Bruce very kindly undertook to prepare a small portion for microscopic examination, without injuring the specimen as a preparation. With this object in view a small piece was cut off. From the cut surface a viscid matter exuded, which formed a flocculent deposit in the vessel, which contained the growth and a solution of chloral. The cut surface afterwards showed a trabecular structure, as shown in Fig. 2. This structure was, of course, still more manifest under the microscope. With the low power a delicate fibrous tissue was seen to enclose spaces of different sizes, while nuclei were present in comparatively small numbers (Fig. 3). When viewed with a high power the delicate texture of the fibrous tissue was still more evident, with here and there a complete connective tissue corpuscle (Fig. 4). Both Dr Bruce and myself were struck by the entire absence of epithelium, both from the exterior of the tumour and the interior of the trabecular spaces. This was most probably due to the treatment of the tumour after removal, although to what stage of the process we were unable to determine. The structure of this tumour seems to correspond exactly with what is described by Zeigler¹ in the following words:—"In loose-textured fibromata a clear juice is contained between the fibrous bundles, which are crossed and plaited and interwoven in all directions." That it could fairly be described as a transition form between mucous polypus and fibroma, I do not think. I have at various times examined many mucous growths from various parts, and have invariably found that microscopic examination showed numerous connective tissue cells, with a delicate undifferentiated stroma. In hard growths the cells gradually elongate into spindles, and in some exceptional cases the whole growth is found to consist of coarse fibrous bundles, showing no tendency to the formation trabeculae, which to my mind is the typical transition. I would also here observe that true myxomatous tissue—i.e., branched cells—is, so far as my experience goes, only met with in sections of mucous polypi occasionally, and then only in small patches.

Some days after the removal of the tumour, a rhinoscopic examination showed the naso-pharynx to be normal, with the exception of a small spot just behind and a little to the right of the upper end of the septum, which was covered with mucopurulent discharge and blood. This I assumed to be the point of attachment of the growth, and being in the least accessible part of the nasal cavity, it accounted for the difficulty experienced in operating.

¹ *Path. Anatomy*, pt. i. p. 196.

III.—PULMONARY PHTHISIS IN CONNEXION WITH OCCUPATION AND WITH OTHER DISEASES.

By ALEXANDER JAMES, M.D., Assistant Physician, Royal Infirmary, Edinburgh.

(Continued from page 945.)

Chronic alcoholism is a condition, the influence of which on phthisis has excited great difference of opinion, being regarded by some as antagonistic, by others as predisposing. In favour of the former view, the fact that alcohol tends to produce an over-growth of fibrous tissue or sclerosis might be brought forward, and on this theory its use has been advocated in the treatment of phthisis, in the hope that encapsulation of tubercular deposits or cicatrization of cavities may be induced.¹

Other observers have, as might have been expected, come to the conclusion that alcoholism predisposes to phthisis. Thus Richardson² has described an alcoholic phthisis, and the elaborate statistics brought forward by Dr Ogle show that the mortality from this disease is high among those whose occupations, etc., imply temptation to over-indulgence in this respect. It must be remembered, however, that although such is the case, the relative mortality in these classes from phthisis may be low, and of this the case of the butchers and cabdrivers may be an example. In these the effects of alcoholism are marked, and the death-rate from phthisis is high, but, as is shown at page 940, the mortality from phthisis relative to the mortality from other causes is low.

That habitual over-indulgence in alcohol, by leading to impaired nutrition, and by increasing the risks of exposure, should often lead to the development of consumption, all must admit, but that alcohol *per se* acts in this manner is a conclusion which the statistics above referred to hardly warrant us to draw.

As regards ague and its connexion with phthisis, a great diversity of opinion exists. In favour of their being an antagonism is a host of names—Wells, Boudin, Graves, Copland, Walshe, Pollock, etc.—and according to these observers this antagonism acts in protecting a phthisical case from malaria, as in guarding a malarious one from phthisis. As to how this influence is brought about, theories are not wanting. Some suppose it to be through the general effects of the malarial poison upon the blood, so modifying assimilation, etc., that the “intrinsic vice of nutrition essential to phthisis shall cease to be a possible existence.” By others it has been ascribed to the enlarged spleen mechanically interfering with respiration; whilst a third supposition might be that it was due to the coincident and subsequent anæmia. Unfortunately, however,

¹ Compare Jaccoud, *Phthisie Pulmonaire*, p. 192.

² Ward Richardson, *Cantor Lectures on Alcohol*, 1875, p. 103.

for the theory of antagonism in general, Hirsch¹ has shown that in the Netherlands, some districts of France, Alsace, Oldenburg, Hungary, South Russia, and Algiers, both phthisis and malaria abound, and in discussing this subject he further suggests that the observation, that with the disappearance of malaria in a district there is the appearance of phthisis, can be accounted for as being due not to the elimination of the fever poison from that district, but to the increase in the density of the population which immediately ensues.² In face of such contradictory statements it is a difficult matter to draw any conclusions, but, as with other matters of disputation, it seems not impossible that an agreement may be arrived at by a compromise, i.e., by admitting that although no general antagonism may exist between these diseases, yet that the effects of the malaria, enlarged spleen, or anæmia, may render phthisis less likely to supervene. The view held by Gowan,³ as the result of his observations of these affections in Siam, is that the enlarged spleen interfering with lung function is the main cause, and in favour of this view he states that he has never seen a case of enlarged spleen and consumption co-existing in which the right lung was not the first to be attacked.

How phthisis may be affected by the enlarged spleen will be discussed by-and-by. Appropriately, however, for consideration comes in now the subject of anæmia. Anæmia is an affection which might reasonably enough be regarded as provocative of phthisis. Caused essentially by more or less prolonged deprivation of fresh air and sunlight, and by improper food, and necessarily associated with deficient physical exercise, it would seem peculiarly likely to induce in the lungs that functional impairment which leads to the development of consumption. As to its connexion with that disease, however, there is perhaps no subject in the practice of medicine upon which a more complete difference of opinion seems to have existed, and to exist. By some it is considered as strongly predisposing as by others antagonistic. So strongly has this latter opinion been held, that, as is well known, Trousseau prohibited the use of iron in all cases of anæmia in which a hereditary tendency to phthisis existed. There are probably few physicians who would carry their views this length, but that a certain antagonism exists between them many will perhaps admit. In favour of such an opinion we have to remember that although the bodily and mental processes in anæmia act feebly, there is comparatively little interference with the vegetative functions, the typically chlorotic being well provided with fat. Further, the dyspnoea which, as the result of deficient hæmoglobin, such patients complain of, must entail increased activity in the lung functions, while the pulmonary con-

¹ *Geograph. Patholog.*, ii. p. 99.

² Compare Vista, *London Medical Record*, 1883, p. 140.

³ Gowan, *Consumption*, London, 1878, p. 57.

gestion and the fact that the blood is more watery than usual, will produce mechanically an excess of water in the lung tissue,—an effect which by many observers is considered to interfere with the process (caseation) on which the development of phthisis is deemed primarily to depend.¹

The fact, further, that with similarly unfavourable surroundings,—deficient light, air, food, and exercise,—one set of cases develops anæmia and another phthisis, seems in itself to denote important constitutional differences, and this opinion has recommended itself to me from the observation, that cases suffering from repeated attacks of anæmia are in time more apt to evidence symptoms of rheumatic or organic cardiac disease than of phthisis. That a family history of phthisis should prevent a physician treating anæmia is, of course, not to be thought of, but the consideration that these affections are brought on by very similarly unfavourable surroundings, and that the latter affection may be to a certain extent protective, warrants us in assuming that we have here a manifestation of symptoms which we should carefully endeavour to interpret before essaying to remedy, and that in the treatment of anæmia the bare administration of iron or arsenic is by no means all that is indicated.

The exanthemata may exercise an influence on phthisis in two ways—first, by the bronchial catarrh with which they are all accompanied; and, secondly, by the debility which they cause, and which lasts after they themselves have passed over. Their effects for evil are, however, not all alike. Typhoid fever is, for example, much more dangerous in this respect than typhus, in explanation of which we have only to remember, first, the greater tendency to bronchitis and pulmonary collapse² which occurs in it; secondly, the possibility of absorption of caseous matter from the intestines; and, thirdly, the fact that its longer duration and not infrequent relapses will produce in time extreme nervous and muscular debility. The extent to which this may reach I have been made conscious of by studying the condition of the muscles in debilitated states of the system. As every one engaged in the physical examination of chests will have observed, there occurs on tapping a muscle—for example, the pectoralis major—in emaciated and weakly individuals a fibrillar contraction and an idiomuscular wave. But occasionally there will be seen running along the muscle from each side of the idiomuscular bulging a small wave, which I do not remember to have seen described clinically.³ It originates, as it were, in the

¹ According to Huss (Reynolds' *System of Medicine*, vol. iii. p. 689), pneumonia seems to be influenced also by anæmia, an antagonism existing between them. He adds, however, that if they do co-exist, the presence of anæmia adds to the danger.

² For the condition of the lungs in typhoid fever, compare Walshe, *ibid.* pp. 229, 316, and 339.

³ By physiologists a wave analogous to this has been described as occurring in muscles after removal from the body (Schiff, *Physiologie der Menschen*, i. p. 26).

stationary idiomuscular bulging, and travels from it towards either end of the muscle at the rate of about one inch per second.¹ This wave is seen occasionally in phthisis; but the best examples of it which I have as yet met with were in cases of protracted typhoid fever. Its occurrence, therefore, in those circumstances, denoting great nervous and muscular debility, is important in connexion with the association of this disease with phthisis, and I would here suggest its possible connexion with those structural changes which Fränkel and Posadsky have described as occurring in the muscles of the larynx, trunk, and limbs in phthisis.²

As might, indeed, be expected in things medical, there are, on the other hand, not wanting observers who consider that not only is there no causal connexion between typhoid fever and phthisis such as above described, but that something of the nature of an antagonism exists between them. Among such are the names of Rokitansky, Rilliet, and Barthez, Cless, Griesinger, Germain Sée, etc. There are, I think, however, few physicians in this country who share this opinion.³

The occurrence of consumption as a sequel to measles is also acknowledged, and according to Copland this is specially apt to occur in the case of individuals about or above the age of puberty. Here, again, the explanation is probably found in the bronchitis and catarrhal pneumonia so apt to occur, and, as Ruehle mentions, in the infiltration and caseation of the bronchial lymphatic glands induced thereby.

The connexion of scarlatina with phthisis seems to have nothing worthy of remark; and as for smallpox, there is an opinion that it is not only not so likely to be followed by phthisis, but that having occurred it may even diminish the tendency to this disease. Thus, Copland says, "I have long remarked, and believe that others have also remarked, the very rare occurrence of phthisis in any one even but slightly marked with smallpox." In practice one often meets exceptions to this, but I mention the opinion as interesting in connexion with the relationship between skin affections and phthisis.

Skin affections, as connected with phthisis, is a subject the investigation of which would seem to be of special interest when the allied functions of skin and lung is remembered. Little about this is known, but there is a consensus of opinion that the so-called diathetic affections, psoriasis, eczema, etc., are very unusual associates of phthisis, the explanation of which is probably the in-

¹ In cases where, as the result of improved nutrition, the gradual disappearance of this wave has been observed, no change in its rate of progress could be detected.

² *Lancet*, 1881, p. 148.

³ Compare Burkart—Ueber miliartuberculose und über das verhältniss der Tuberculose ueberhaupt zum abdominal typhus.—*Deutsches Archiv für Klinische Medicin*, xii. p. 277.

creased tendency to perspiration and the functional activity of glands, etc., which this disease induces.¹

The parasitic and contagious variety of skin affections are, of course, found in consumption, and of these notably pityriasis versicolor, but whether the nature of the skin and sweat in consumption present conditions peculiarly favourable for the implantation of the tinea, or only favour its development by precluding strict attention to cleanliness, is not clearly settled. It is certainly much more common among hospital or dispensary than among private patients.

Diabetes mellitus is well known to be closely associated with phthisis. It occurs often as a forerunner, and as an item in the family history its importance is acknowledged.

Why the diabetics are specially liable to phthisis has been explained in various ways. By some it has been ascribed to their state of nutrition being much below par, by others to the probability of the trophic function in the lung being impaired owing to disturbance in the vagus innervation;² whilst a third view is that owing to dryness of the lung tissue the process of caseation will be favoured.

It is also noteworthy that, according to Leichtenstern³ and others, the percentage amount of hæmoglobin in the blood in diabetes is occasionally quite up to, or even above the normal, owing probably to the relative deficiency of water. If anæmia, with its relative deficiency in hæmoglobin, tends to produce pulmonary congestion and consequent moisture, we might suppose that we have in this a further cause for the production of a dry state of the pulmonary tissue in diabetes. In whatever way produced, however, there can be no doubt that in diabetes tissue nutrition is peculiarly abnormal, simple pneumonias in such patients often terminating in gangrene.

As regards etiology, however, there are some important points of difference between it and phthisis. Thus it is more common in males than in females; and further, although most frequent in early life, yet a large proportion of cases occur after the 45th year.⁴ Again, the corpulent frequently become diabetic. It is certain, however, that in advanced life and in the corpulent the prognosis is always much more favourable.

Another condition which has been said to be an infrequent

¹ It is of interest to note that asthma has been observed by Sir Andrew Clark and others to be associated with skin affections in such a way as to indicate some relationship.—*Brit. Med. Journal*, Nov. 21, 1885.

² Schou has found in the alveolar and pleural exudation of vagus pneumonia a micro-organism which, when administered by tracheal injection or inhalation to a healthy animal, seems capable of inducing in it a similar inflammatory affection.—*Centralbl. für Klinische Medicin*, Jan. 23, 1886.

³ Untersuchungen über den hæmoglobinhalt des blutes in gesunden und Kranken Zuständen. Leipsig, 1878.

⁴ In the ten years (1870 to 1879) there occurred in Scotland 633 male and 412 female deaths from diabetes. Of the male deaths 56·25 per cent., and of the female 57·27 per cent., were before the age of 45.

associate of, or indeed antagonistic to phthisis, is enlarged external lymphatic glands,¹ and when we remember that such glands are believed by many to be really tubercular foci, such a statement seems somewhat remarkable. In connexion with this, further, the infrequency of phthisis in individuals the subjects of open strumous sores has been noted. Pollock says, "It may begin before the glands are affected or after their cicatrization, but it is not often found to begin with their suppuration."

There can be no doubt, however, that there is here only the appearance of an antagonism. In the first place, the tendency to enlargement of the cervical glands in strumous individuals occurs at an earlier age than that of phthisis; and, secondly, when phthisis does develop in an individual with such glands, these, if there be any fever, will be sure rapidly to diminish.

Again, the fact of phthisis being uncommon in those suffering from suppurating glands, may (apart from any counter-irritating effects) be ascribed to the chances of resorption of tubercular matter being then reduced.

The bearing of these considerations on the treatment of glandular affections is obvious, but need not be discussed here. At present it is only necessary to notice that in connexion with the etiology of phthisis, enlarged lymphatic glands are anything but antagonistic in the real sense of the word. They are not only the manifestations of the same constitutional weakness, but may act as foci for tubercular infection.

We now pass to the consideration of phthisis in connexion with other affections of the lungs themselves, and, as might be expected, the information that meets us here is decidedly more definite. All observations agree in showing that whatever induces what has been termed *venosity* renders phthisis less liable to occur, but whether this is the result of the increased respiratory function, the pulmonary congestion and consequent moisture of the lung tissue, or other causes, is of course not certain. The conditions which give rise to this venosity are manifold, and merit detailed consideration.

Organic disease of the heart is rare as an associate of phthisis, and when it does occur it seems to favour slow progress. The infrequency may here be due to the fact, as stated by Walshe, that heart affections and phthisis are prone to occur at different periods of life, but that the venosity is really an important element in this antagonism is evidenced by the observation that mitral stenosis is the most, and aortic valvular affection the least rarely, a complication of consumption.² When we further reflect that the diathetic conditions in rheumatism and phthisis are closely allied, and that mitral disease is more associated with the early years of life than aortic, our opinion in this matter is strengthened.

It is to be remembered here that symptoms and signs of cardiac

¹ Walshe, *ibid.*, p. 435, and Pollock, *ibid.*, p. 90.

² Ruehle's article on Consumption in Ziemssen's *Cyclopaedia*, p. 508.

and vascular disturbance are common in phthisis, apart from organic disease, in the form of hæmic murmurs, and murmurs the result of a twist or kink, or dilatation of the large vascular trunks, produced by cicatricial contraction of an affected lung. Such conditions are usually, however, readily distinguished from organic disease.¹

A reference must also be made to the co-existence of true aortic aneurism and phthisis. Aneurism, like most other cardiac or vascular lesions, tends in time to produce pulmonary congestion, and in this respect has been regarded as antagonistic, but by pressure of the sac on the lung a symptomatic and pathological condition, identical with phthisis, is brought about, and for this, as Balfour² notes, the patient often seeks relief unconscious of his real malady.

Along with organic cardiac or vascular disease are to be classed all congenital malformations, persistent ductus arteriosus, etc. These have been stated to be antagonistic to phthisis for reasons similar to those given above.

Malformations of the chest walls, the result of rickets, chicken breast, spinal curvature, etc., have been by most observers rarely found in phthisis. This is, of course, to be ascribed to the consequent venosity, of the existence of which in severe cases the cyanotic complexion so frequently presented, and the liability to bronchial catarrh, from which such cases as frequently suffer, afford abundant evidence.

Abdominal affections, which impair the play of the respiratory apparatus, as ovarian cysts, for similar reasons are to be considered as antagonistic, and under this category comes also the enlarged spleen of ague. Pregnancy, it is to be noticed, does not diminish the respiratory capacity to anything like the extent which might have been supposed, still by many it has been considered to exercise a beneficial effect in this way.

A rather important and very interesting question as to the influence of a large waxy or fatty liver on the course or development of pulmonary phthisis comes up here for consideration. Seeing that, let us say, waxy disease and phthisis are found in similar constitutional states, and that in the latter affection the waxy liver is a not infrequent complication, can we say anything as regards its influencing, in a manner which theoretically we might suppose it to be capable of doing, the pulmonary condition? There is no doubt that although the patient's general condition is worse after the development of the amyloid disease, yet the pulmonary symptoms often recede. How much of this is due to the mechanical effect of the large liver on the respiratory organs, and how much to the general effect of the complication, hydræmia, dropsy, diarrhoea, it is, however, impossible to say.

Lastly, we have to notice in connexion with conditions producing venosity that Rokitsansky says (p. 318), "Congenital smallness of

¹ Compare Brakenridge, *Lancet*, 1880, i. p. 80.

² *Diseases of the Heart*, 2nd edition, p. 380.

the pleural sacs, paired with primitive smallness of the lungs, and, as it mostly is, with an inverse ratio of the development of the abdomen and its viscera, serves as a protection against tuberculosis." With this one has to remember the converse, viz., that phthisis often occurs when the chest, as regards size, etc., is specially well developed, and when the respiratory capacity, as ascertained by the spirometer, is correspondingly great.

The connexion of bronchitis with consumption has given rise to a great amount of discussion, and upon it views of the most contradictory nature have been expressed. These we need not attempt to consider in detail; it is sufficient to mention that whilst some, as Louis and Laennec, have held that a bronchitis never leads to tubercle, others, as Graves and Niemeyer, assert that the direct opposite of this is much nearer the truth. With the latter most physicians will agree, but for the very decided opinion expressed by such as Louis and Laennec there is a certain foundation. In the first place, we must remember that phthisis may pass through its premonitory and first stages without cough as a marked symptom; and, further, that in comparatively advanced stages cough may be absent for days or weeks at a time. Secondly, the ages at which bronchitis is most frequent and apt to produce fatal results are different from those of phthisis, as the Registrar-General's return shows the mortality from the former is greatest during the very early and very late years of life. Thirdly, the tendency which long-standing bronchitis has to induce emphysema indicates an antagonism of a kind. Having said this, however, all that can be evidenced in the way of an antagonism has been said. A neglected bronchial catarrh is a very frequent cause of consumption, and it might not unreasonably be supposed that the infrequency of death from bronchitis in the adult, as compared with the early and late years of life, which the Registrar's returns show, is due to the possibility that, as the result of the bronchitis, phthisis has then supervened, and proved fatal.

When, in consequence of the bronchitis, emphysema has occurred, the tendency to phthisis is of course diminished.

Pneumonia is a disease which, from the consideration of its pathological condition, and the age at which it is most apt to supervene, might readily be looked upon as closely allied to phthisis. But by many, and more especially in Germany, the very opposite of this is held to be the case. Thus Heitler,¹ after discussing the possibility of a croupous passing into an interstitial pneumonia, says, "auf diese weise entstand die lehre von dem ausgange der kroupösen pneumonie ins kasige phthisie, eine lehre welche heutzutage kaum mehr einen warmen vortheidiger finden will." Nevertheless instances, real or supposed, of such a termination are often published even in Germany,² and in this country a possible connexion between the two

¹ Heitler, *Wiener Medicinische Wochenschrift*, 1884, p. 1530.

² Schultz, *Deutsche Medicinische Wochenschrift*, 1885.

is by most physicians acknowledged. Between them there are, however, essential differences.

Thus croupous pneumonia is very rarely immediately followed by phthisis of the lungs; in any case where it is so, a strong predisposition must undoubtedly have existed. It would seem, also, that if it supervene in the course of this disease, resolution may occur as readily as it does in a healthy individual, and no marked increase in the severity of the phthisical process may follow.

Again, croupous pneumonia usually affects the lower lobes, and differs still further from phthisis, in that whilst the pneumonic process is usually limited by a lobe, the phthisical is usually not, but, as physical examination shows, extends from the apex downwards, often involving the middle lobe or upper part of the lower lobe posteriorly, before spreading to the whole of the upper lobe, the greater part of which is anterior.

Further, the pneumonic process commonly affects lungs or portions of such already congested, as in heart disease, an occurrence the reverse of what we have seen to obtain in phthisis, and with this also, it is to be remembered, that in high altitudes where phthisis is rare pneumonia is common.

Thus, then, certain distinct differences between these diseases are to be met with. From a clinical point of view, however, there is no hard and fast line; for an apex pneumonia, or one which, because occurring in a debilitated or cachectic individual, is catarrhal rather than croupous, may be the beginning of a phthisis. It would be interesting to know, presuming that it is possible, what percentage of cases of acute pneumonia seems to lead directly or indirectly to such a termination. Such information cannot be easily obtained, but all observation seems to point to the rarity of this event. Out of 170 cases of phthisis, the histories of which I have had the opportunity of carefully investigating, I find that although in 18 of them was such a commencement possible, in only 3 could it be said to have been at all probable. In 2 out of the 18 there had been attacks of pneumonia years previously, from which, however, complete recovery had occurred.

Pleurisy seems to be very closely related to phthisis. It occurs frequently during its course, but what is of special importance in connexion with its etiology is that it occurs with very great frequency in individuals who, weeks, months, or years afterwards, become phthisical. As was the case with pneumonia, it is difficult to give with anything like accuracy the proportion of cases of pleurisy which end, directly or indirectly, in the development of phthisis; but of the 170 cases above referred to, in which the personal history had been carefully inquired into, certainly 9 had suffered from and dated their illness to this disease. In several of these there had been repeated pleuritic attacks; in one the first attack had occurred thirty years before the onset of the phthisis; in most, however, their occurrence had been from one to three years previously.

But now arises the question, In what way are these diseases associated, i.e., how is it that many individuals who have had pleurisy tend to become phthisical?

In the first place, we must remember that the occurrence in an individual of one or several pleuritic attacks may be regarded as evidence, as strumous glands, catarrhs, etc., are, of a constitutional debility which in time will lead to the development of phthisis.

But, secondly, the connexion may be more direct. A lung which, as the result of an incompletely absorbed pleuritic effusion, is semi-collapsed, or one which, with dilated bronchial tubes, is struggling to lessen the retraction which the chest parietes have to undergo as the result of a too long delayed absorption, is certainly not in a healthy state, and might, from the lessened movement, the tendency to bronchitis and retention of secretions,¹ be regarded as being in a fit state for the development within it of the phthisical process.

Thirdly, the air cells lying beneath the inflamed pleura may become involved by continuity, and the exudation into them becoming caseous may lead to phthisis.

Or, fourthly, the pleuritic effusion, in consequence of the process of absorption, for one or another reason, being incomplete, may undergo a similar caseous transformation, and cause phthisis by resorption of this caseous material, i.e., by autoinfection.²

In either of these ways, then, the connexion of pleurisy with phthisis may be explained. As to which, however, obtains most frequently in practice there may be some difference of opinion, as in many cases of pleurisy followed by phthisis all may have existed together.

Full information as to the part of the lung and the side affected in cases of phthisis which have previously suffered from pleurisy would be interesting. Such is, however, difficult to obtain, as patients' accounts of pleurisies which had occurred months or years previously are not of much value. Of such cases, however, I have collected 8, in which the data are trustworthy:—

1. M., æt. 43; pleurisy (with effusion) of left side, terminating in complete recovery, but followed two years afterwards by phthisis of right apex.

2. M., æt. 35; pleurisy (with effusion) of right side, terminating in complete recovery; six years afterwards pleurisy of left side, terminating again in recovery, but after the lapse of another year followed by phthisis of left apex.

3. C., æt. 27; pleurisy (with effusion) of left side, terminating in recovery; ten years afterwards phthisis of left apex.

4. L., æt. 23; pleurisy (with effusion) of right side; recovery incomplete, and one year afterwards phthisis of right base.

¹ Sir Andrew Clark, *Lancet*, 1885, p. 651.

² Jaccoud, *Phthisis Pulmonaire*, p. 81. Compare Lustig, *Wiener Medicinische Wochenschrift*, 1884, p. 52, for a case of traumatic pleurisy which became tubercular, the lungs being entirely unaffected.

5. W., æt. 32; pleurisy of left side, terminating in recovery; three years afterwards phthisis of both apices, most advanced on left.

6. C., æt. 34; pleurisy, slight, of left side, and immediately followed by marked pleuritic effusion on right; recovery, but four years afterwards phthisis of right apex.

7. H., æt. 25; pleurisy (with effusion) on right side; recovery; fifteen years afterwards phthisis of right apex.

8. F., æt. 40; pleurisy (with slight effusion) on left side, terminating in recovery; four years afterwards phthisis of right apex.

These cases are numerically insufficient to afford us any trustworthy data. It would seem, however, probable that the phthisis which follows a pleurisy which has been completely recovered from does not necessarily affect the lung of the corresponding side. Cases must, however, be judged individually.

IV.—SUBJECTIVE SYMPTOMS IN EYE DISEASES.¹

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IV. NIGHT-BLINDNESS.

AT one time it was supposed that that peculiar form of defect of vision which either only makes its appearance towards night, or becomes at any rate much more aggravated at that time, was, as it were, a kind of periodic weakness resembling ague in its nature. When, however, the attention of modern ophthalmologists was directed to the subject, night-blindness was soon found to be merely the expression of a defect in the light sense, a greater intensity of illumination being in general necessary in order that individuals in whom this defect exists should be capable of attaining their full visual acuity. There is an unfortunate confusion as to the proper scientific term which should be accepted as synonymous with night-blindness. In the most recent English text-books the word *nyctalopia* is so used, whereas on the Continent *hemeralopia* has come into such general use that little or no attempt seems to have been made to change to the older, and from an etymological point of view more strictly correct name.² For this reason it is perhaps better to employ the English word *night-blindness* alone.

Physiologically there exists a condition in some respects allied to night-blindness. Thus, if we suddenly enter a darkened room after our eyes have been exposed to the full light

¹ Continued from p. 733.

² We have the authority of Galen for deriving *nyctalopia* from *νύξ* and *ἀλας*, and not from *νύξ* and *ὄψ*, and therefore using the expression to signify night-blindness, and not night vision or day-blindness, as is often done.

of the day, we experience at first a much greater difficulty in recognising objects around us than we do after the lapse of some minutes spent in the comparative darkness. What is called an adaptation of the retina takes place gradually, and is the longer in attaining its maximum the more intense has been the illumination to which the eyes were previously subjected. The time necessary for complete adaptation is also subject to individual differences depending on the state of health, etc. If the eyes be subjected continually day after day to a more than usually intense light, such as is reflected from the surface of the sea or plains in tropical climates, or from chalk pits or snow, there is apt to be set up a condition which has been called *idiopathic night-blindness*.¹ This is more especially liable to occur if, along with the exposure to strong light, the individual is the subject of some weakness—malnutrition, anæmia, scurvy, etc. And although in much the greater number of cases of idiopathic night-blindness the primary exciting cause has been found to be referable to the action of strong light alone, or combined with such conditions as those mentioned, a certain proportion of cases appear to occur as a result of these debilitating circumstances independently of any abnormal conditions of illumination. Of this nature are the cases described as occurring occasionally in women shortly before confinement, and in some cases of cirrhosis of the liver and jaundice. The few cases which I have seen of idiopathic night-blindness, which is much more common in the southern countries of Europe and in the tropics, have been in sailors and soldiers who have returned from these regions. The affection apparently begins with a condition closely resembling that just described as physiological, but in which the period occupied by the adaptation of the retina is very much prolonged. Eventually the adaptation has not time to become complete before the individual is again subjected to the unfavourable conditions, and so a true night-blindness is more or less gradually acquired. The difficulties of vision come on during dusk, and in rooms illuminated by artificial light. Unless the illumination is pretty powerful, only objects directly illuminated by the source of light are seen distinctly, the others being more or less indistinct according to the severity of the symptoms. The condition is one of anæsthesia or torpor of the retina, which requires an abnormally strong stimulus to awaken its physiological activity. Other symptoms are found to co-exist depending more or less on the same cause. Thus there is often, and to an extent varying in different cases, a restriction of the field of vision; there is, too, a certain amount of defect in the vision for blue, which leads to a tendency to confuse between shades of blue and green, a

¹ The condition arises, then, from the illumination of the whole retina by strong irregularly reflected or scattered light; other symptoms, viz., a central scotoma, due to coagulation in the retina, are caused by the direct action of the light from strong sources of illumination, such as the sun or electric arc.

condition which is not characteristic of any form of true congenital colour-blindness. Alfred Gräfe has also described as complications defective accommodation and convergence, as well as a diminished tendency towards the fusion of double images produced by prisms. These are probably due to defective stimulation, to reflex or associated actions, owing to weakness of the afferent stimulus. They disappear along with the night-blindness. According to Reymond, the visual acuity of individuals suffering from idiopathic night-blindness does not begin to diminish until the strength of the illumination is reduced to a point which begins to tell on the normal acuity, but from this point there is a very much more rapid deterioration than in normal eyes as the illumination is further reduced. The indication for treatment is to withhold light so as to allow the retina time to recover itself, and in complying with this indication it is not necessary, as some have advised, to keep the patients in absolute darkness, but merely in considerably subdued light, either by the use of dark spectacles or residence in a darkened room, while at the same time attention should be directed, if necessary, to means calculated to improve the general health. Treatment carried out on these lines is always successful, and usually after a very short time. There appears, however, to be a decided tendency to relapse, which should be guarded against by a prolongation of the treatment after recovery. In a considerable proportion of cases of idiopathic night-blindness there is also a condition of xerosis of the conjunctiva due to the glare which gives rise to the defect of vision.

The form of night-blindness just described is accompanied by no marked or constant ophthalmoscopic changes.¹ This is also mostly the case with the somewhat rare condition of congenital night-blindness, although it is highly probable that it is closely allied to the degenerative change of the retina known as retinitis pigmentosa, possibly the same disease occurring during intra-uterine life. Cases of congenital night-blindness have not, however, generally been found to be complicated by restriction of the field of vision, so that they may possibly be due to some defective development, a view which seems to be supported again by the fact that they remain stationary during life.

Much the most common causes of pathological night-blindness are conditions which, in most cases at least, produce visible ophthalmoscopic changes. Of such, again, retinitis pigmentosa is perhaps the most common, and certainly the most important. In this disease night-blindness is, indeed, usually the most prominent symptom, and the one which first attracts the attention of the patient or his friends; although complicated by other symptoms, chief of which is the almost constant diminution in the extent of

¹ According to Poncet there is constriction of the arteries and dilatation of the veins of the retina, conditions which, if they exist at all, have certainly little to do with the characteristic symptoms.

the field of vision,¹ which is more and more marked the feebler the illumination. There often remains for a long time an excellent and sometimes perfect acuity of central vision under a good illumination. The defect of peripheral vision renders the difficulties from which patients suffering from this disease experience in comparative darkness all the more marked and distressing. They are able to go about perfectly well by themselves during the daytime, while they have often to be guided as soon as it is dusk. Unfortunately there is no cure for this disease, and therefore for the accompanying night-blindness—the changes occurring, though often slowly and even intermittently, being always in the direction of deterioration, and not infrequently leading to complete blindness. In some cases of retinitis pigmentosa, when the vision has become very defective, the night-blindness is not marked, and sometimes even gives place to a kind of day-blindness, but I am not aware that this unusual symptom has ever been met with where there was good central vision. In most cases which I have seen for the first time in adults there has been night-blindness for long previously; occasionally, however, there is a distinct statement as to its having come on recently, so that although the disease usually begins in childhood, it certainly sometimes first makes its appearance in adult life. A not uncommon form of choroido-retinitis, in which there is also, as in retinitis pigmentosa, a deposition of the pigment of the hexagonal retinal cells in the more superficial layers of the retina, and which is usually if not always of syphilitic origin, is also associated with night-blindness. I have never, however, seen the night-blindness so pronounced in this inflammation as it is in the cases of true retinitis pigmentosa, which are sometimes confounded with it, and which are more of the nature of a degeneration or sclerosis than an inflammation in the limited sense of the term. Acute diffuse choroiditis, too, produces the most decided diminution of the light sense, and therefore night-blindness; this symptom is very evident, too, in some cases of detachment of the retina (idiopathic detachment), but not so much so either in the disseminated inflammations of the more peripheral parts of the choroid or the central choroiditis of old age. A certain degree of night-blindness, often sufficient to be rather inconvenient, is characteristic of the high degrees of myopia when associated with choroidal changes. Koenig has also described two interesting cases of defective development of the choroid, in which night-blindness was a marked symptom although the central vision was good.

When we come to inquire what is the pathological condition which gives rise to night-blindness, we must evidently look, in the first place, for some change which is common to all the affections in which it is met, and which at the same time is sufficient to explain the cases where the night-blindness is idiopathic and unaccompanied

¹ *Vide Examination of Field of Vision.*

by alterations sufficiently grave to be visible with the ophthalmoscope. It is of course possible, though not perhaps so probable, that different conditions may give rise to the same symptom. Thus it has been supposed, that as although most cases of idiopathic night-blindness have resulted from more or less prolonged subjection to diffuse light, still as undoubtedly the same defect may arise under such conditions as have been mentioned without the aid of that factor; the immediate cause may lie at one time in the retina, and at another in the nerve centres. Again, it has been ascribed to the limitation of the field of vision, and undoubtedly this limitation adds somewhat to the difficulties of night-blind patients, but it is more the condition of night-blindness itself which, by causing the more pronounced limitation of the field of vision in subdued light, gives rise to these exaggerated difficulties, than the defect of the peripheral vision, which brings about night-blindness. That this is the case is evident from the fact, that a good many cases of night-blindness are unassociated with constriction of the field of vision, and, besides, some forms of disease exhibit often a very pronounced restriction of the field, without producing at the same time the symptoms under consideration. Indeed, this is constantly the case when the restriction of the field is symptomatic of primary atrophy of the optic nerve fibres. Now, the experiments of Charpentier and others have demonstrated pretty conclusively that there are two distinct retinal end-organs, through the medium of which the transformation of physical states into the nervous stimuli leading to vision take place. One of these merely effects the transformation of energy, which gives rise to the perception of light, and is therefore, so to speak, the end-organ for the light sense; the other is capable of differentiating the impressions formed on it or on the former (or it may be the difference of the impressions formed on the two together), so as to lead eventually to a consciousness of the varying intensities and quality of the light rays falling on different parts of the retina, and thus give rise to the sensations of form and colour. The physiological night-blindness, so to speak, to which I have referred, shows that on exposure to light there is produced a certain degree of exhaustion of the light sense end-organ, so that a certain time has to elapse before it recovers itself sufficiently to be capable of its full delicacy. We have seen, too, that abnormal stimuli are capable of very much intensifying the state of exhaustion, and consequently prolonging the period necessary for recovery. Thus it becomes a mere question of the balance between supply and demand, so that it is evident that an abnormal degree of exhaustion may be occasioned by either an excessive demand on the one hand, or defective supply on the other. There is, therefore, no occasion to relegate to different categories of causation the idiopathic night-blindness caused by strong light, and that resulting occasionally from debilitating disease or altered conditions of the blood, as both may evidently be due to exhaustion in the

same end-organ. What that end-organ probably is we learn by a study of the more distinctly pathological conditions associated with the symptoms of night-blindness. We find, then, that wherever there is absence or destruction of the retinal pigment cells, be it congenital or the result of inflammatory or degenerative changes, there night-blindness is a more or less marked symptom, and this independently as to whether other elements of the retina are affected or not. In the case of retinitis pigmentosa the symptoms are necessarily complicated, as apart from the disturbance of the retinal pigment there is the sclerosis of the vessels and connective tissue of the retina leading to its degeneration, and it is probably owing to this circumstance, as it is in connexion with this disease, that the symptom of night-blindness has been principally studied, that the cause of that symptom remained for so long obscure. Not only, however, have the more recent investigations into the physiology and pathology of the light sense clearly pointed to the hexagonal pigment cells as the end-organ for this factor in vision, but they indicate a method of examination by which in doubtful cases we may localize the cause of visual defects, and so determine whether they are of central or peripheral origin.

A degree of night-blindness is complained of by miners suffering from the condition of nystagmus, to which they are subject. This has not been referred to above, as it is certainly due to a different cause from that which gives rise to what may more correctly be called true night-blindness. It arises, in fact, merely owing to the increased nystagmus which is occasioned by the greater difficulty of seeing objects in the dark, which is common to all individuals.

V. DAY-BLINDNESS.

Although a considerable number of cases of day-blindness, in which vision in ordinary daylight is very imperfect, though of normal acuity, or at any rate much better in subdued light, are on record occurring not only in isolated individuals, but endemically, yet this subject has not yet received a proper scientific investigation, and it is not known what is the exact nature of the disturbance which gives rise to this curious perversion of vision. It is said to arise in hot climates under much the same conditions which give rise to the much more frequent symptom of night-blindness, and to be some form of retinal fatigue. It is much more probably, however, allied to cases which indicate a degree of hyperæsthesia of the retina. There are few distinctly pathological conditions in which the symptom of day-blindness pure and simple is at all marked, so that on this account, too, it is difficult even to conjecture what may be the cause of idiopathic day-blindness. Cases of abnormal sensitiveness to ordinary daylight are indeed commonly enough observed, and such sensitiveness may be greatly increased by the habit of wearing darkened glasses, or keeping much in darkened

rooms. How far such cases, in some of which there appears to be, along with the more prominent symptoms of hyperæsthesia, an actual improvement of vision in subdued light, are to be considered as analogous to day-blindness, does not appear very clear. Prisoners who have been kept for years in comparative darkness have been said to have become day-blind. Yet here again it is not easy to determine from the descriptions given how far the symptoms exhibited were those of hyperæsthesia alone. Albinos often have more difficulty in seeing in a bright than in a subdued light, owing to the excessive glare which the comparative absence of pigment in the iris and choroid permits of. The same difficulty, though for the most part much less marked, is seen when there is from any cause excessive mydriasis. When there is central amblyopia due to diminished functional activity of the nerves supplying the central region of the retina, much the most common exciting cause of which is the toxic effect of tobacco, patients often state that there is an improvement in their vision when the surrounding light is subdued. In cases which I have specially examined with reference to this point, I have not noticed any actual increase in visual acuity when the light has been diminished, but rather the reverse, so that it is probable that such a condition is merely associated with a greater sensibility to glare, which might cause what is so often complained of, the appearance of objects being less distinct in a bright than in a subdued light. The only cases which, so far as I have observed, present symptoms resembling true day-blindness are some cases of reflex or hysterical amblyopia, in which, if one can trust the statements of the patients, there seems to be a more or less distinct improvement of vision in subdued light. In one case, in which there was a marked restriction of the field when tested by ordinary daylight, I found the peripheral vision normal in extent in a darkened room. But these cases more frequently present the symptoms of anæsthesia of the retina, and evidently from a different cause from that which has been explained as giving rise to night-blindness. The constant irritation of sensory nerves in different parts of the body, which produces in them the greater or less degree of amblyopia, to account for which no ophthalmoscopic changes can be found, does so by unconsciously alienating the attention from the impressions produced on the visual centres, when images are formed on the retina, just as the absorption of the attention by the impressions of one or more senses may make us unconscious of the stimuli produced on the centres of other senses. And that such is the case is shown not only by the fact that reflex amaurosis and amblyopia may be suddenly cured when the source of primary irritation has been removed, but also that in these cases the pupil is not generally inactive, as it is more or less when the cause of the blindness is peripheral to its reflex centre. Such cases suggest an explanation of day-blindness. We have said that this condition is allied to hyper-

æsthesia of the retina, and it is not unlikely that this hyperæsthesia, in which this action of light produces a kind of irritation of sensory nerves either in the retina or somewhere in connexion with the nervous apparatus of vision, and which appears to be engendered by the subjection of the eyes for longer or shorter periods to brilliant light, may give rise to much the same kind of blindness as that to which we commonly give the name of reflex amblyopia, with this difference only, that as the original source of irritation proceeds from the visual apparatus, and is caused by light itself, it would be less, and therefore admit of better vision when the illumination was diminished to an extent which rendered the irritation less perceptible. That is, there would be likely, in cases where the hyperæsthesia existed without so much pain as to give rise to the spasmodic closing of the eyes, to be an improvement of vision in subdued light or, in other words, day-blindness.

V.—ON THE NATURE AND CAUSE OF THE MOVEMENT OF INTERNAL ROTATION.

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(Read before the Edinburgh Obstetrical Society, 10th March 1886.)

ALTHOUGH internal rotation is one of the most striking movements of the part engaging in the pelvis during parturition, we are yet in want, not only of a reasonable explanation of its cause, but also of a detailed description of its nature in the various presentations, normal and abnormal, in which it happens.

In bringing a contribution to this subject before the Society, I purpose considering it under the following divisions:—

1. THE HINDRANCES TO A RIGHT CONCEPTION OF THE CAUSATION OF THE MOVEMENT, DUE TO ERRONEOUS ANATOMY AND DESCRIPTION.
2. THE ANATOMY OF THE PARTS CONCERNED IN BRINGING ABOUT ROTATION.
3. A DESCRIPTION OF THE MOVEMENT OF INTERNAL ROTATION IN THE VARIOUS PRESENTATIONS.
4. THE CAUSATION OF THE MOVEMENT OF INTERNAL ROTATION, GENERAL AND PARTICULAR.

1. THE HINDRANCES TO A RIGHT CONCEPTION OF THE CAUSATION OF THE MOVEMENT, DUE TO ERRONEOUS ANATOMY AND DESCRIPTION.

Our knowledge of the cause of rotation has been greatly retarded by the assumption that it must be due to some shape of the bony

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FIG. 1.

DIAGRAM OF ROTATION
in Head Cases.

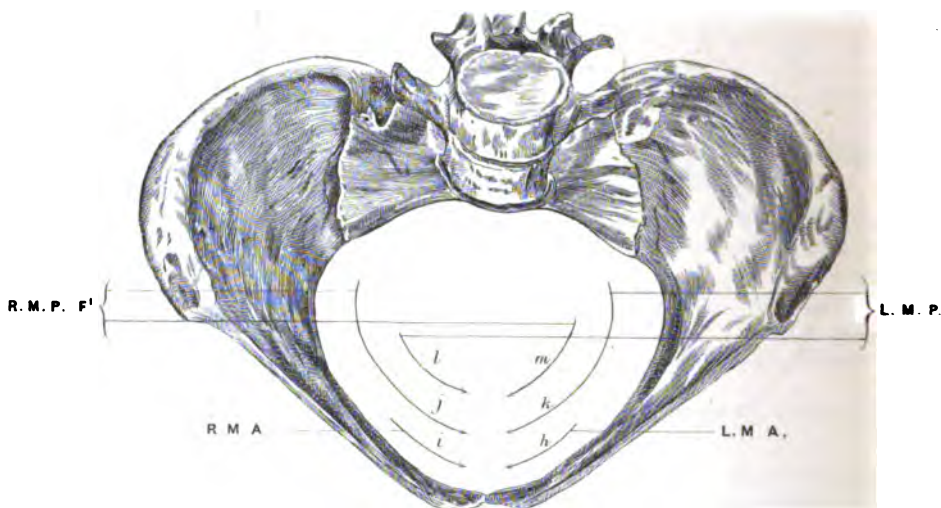
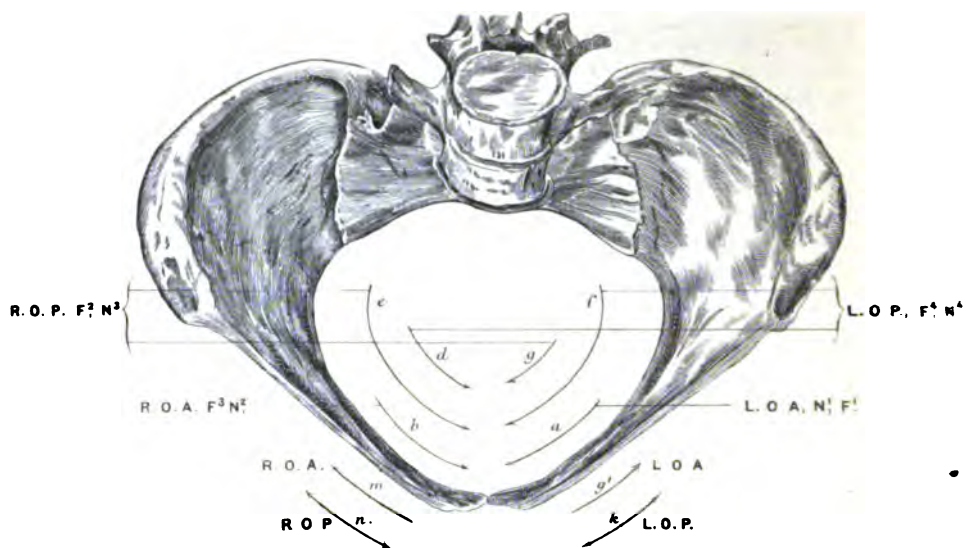


FIG. 2.

DIAGRAM OF ROTATION
in Face Cases.

FIG. 1.

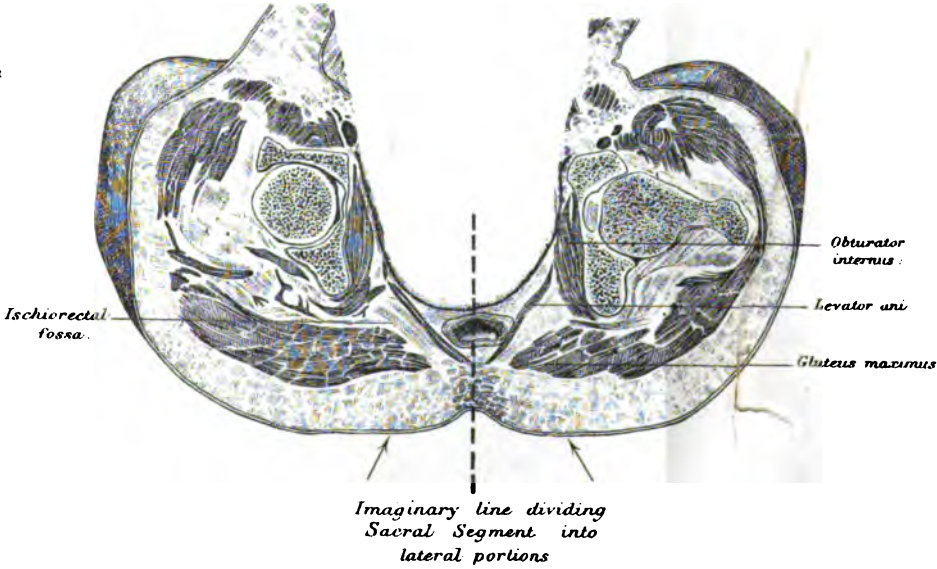
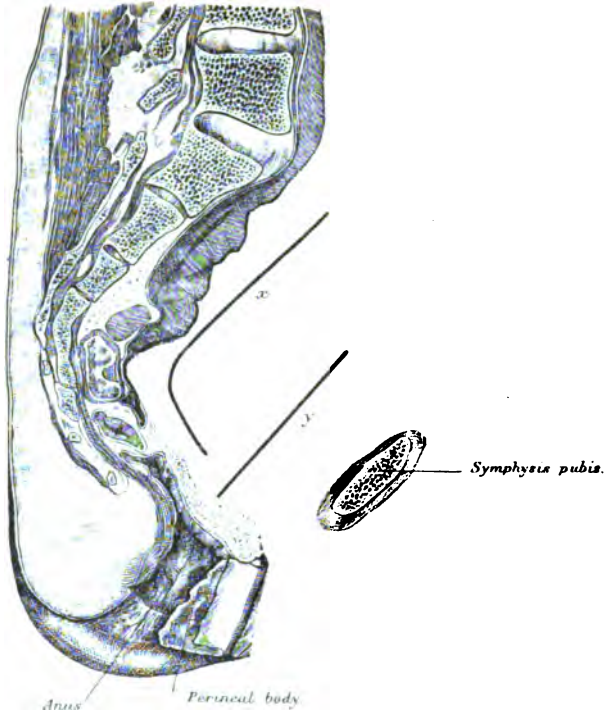


FIG. 2.



pelvis. The tissue lining the pelvis has been regarded as having little influence, although in German text-books the pressure of the tissue lining the posterior wall of the pelvis has always been held to be a factor in determining it. Now, mere change of shape in the bony pelvis will not cause rotation; the only result can be a distortion of the head, *i.e.*, the head will mould itself to the varying capacity of the bony pelvic walls, but not necessarily rotate. This holds even more strongly of the breech. Another very misleading term has been posterior rotation of the occiput in head cases, or chin in face presentations. As I hope to show, no part in the pelvis ever rotates primarily backwards. Rotation is always a forward and inward movement.

2. THE ANATOMY OF THE PARTS CONCERNED IN ROTATION.

This must be considered in relation to—

(a.) *The Canalized Pelvic and Pelvic Floor.*

(b.) *The Fœtal Body, especially the Head.*

- (a.) *The Canalized Pelvis and Pelvic Floor.*—This may be considered as made up of two parts, one in front of the transverse of the brim and true pelvis, the other behind it. The first is the anterior half of the pelvis and tissues, the second is the posterior half with its tissues.

- During the second stage of labour the anterior half of the canalized pelvis is practically the same as that of the bony pelvis, inasmuch as the bladder and retropubic fat are partially drawn out of the pelvis, and it is lined symmetrically with vaginal and cervical tissue.

- The posterior pelvic wall, chiefly sacrum and coccyx, is different, however, inasmuch as it has springing from it a segment of the pelvic floor, the sacral segment, attached to the sacrum, coccyx, and edges of the sciatic notches.

This sacral segment is the most important factor in bringing about rotation, and as such must be fully considered.

- It is fixed above to the sacrum and coccyx, extends downwards from the bony outlet of the pelvis posterior to the ischial tuberosities, and is unattached at the lower end. It thus includes the posterior vaginal wall and tissues behind it, and has imbedded in it parts of the obturator internus, coccygeus and levator ani muscles, as well as the transversus perinei. Part of the strong gluteus maximus also enters into its formation (Figs. 1 and 2, Plate II.)

From its attachments it can be pulled back and made to recoil, *e.g.*, by Sims' speculum, or driven back by the part of the fœtus engaging in the pelvis. It is the strong segment of the pelvic floor, as will be understood by the following measurements:

Length from tip of coccyx,	.	.	3 inches.
Thickness at level of cervix uteri,	.	.	2 inches.
Do., at anus,	.	.	2 inches.

The length is of course increased during labour. The sacro-sciatic ligaments, coccygeus, and levator ani muscles all help to restrain undue driving back of the lower portion of the sacrum and coccyx as well as of the sacral segment.

It is important to consider it divided into two portions—a right lateral and left lateral—lying respectively to the right and left of an imaginary vertical mesial line on the lower portion of the sacrum coccyx and posterior vaginal wall (Fig. 2, Plate II.) The anterior pelvic wall may be considered as the concave aspect of a segment of a circle, on part of which the rotating head or body glides.

One specially important point to be kept in mind is that the anterior and posterior walls of the canalizing pelvis are of very unequal lengths, and that consequently the anterior portion of the presenting part at the brim will touch the sacral segment, and be compressed or pushed forward by it very much sooner than the presenting part lying posteriorly. The recoil of the sacral segment is increased as we pass towards its lower end, as it acts like a lever of the first kind (Fig. 1, Plate II.)

b. The Fœtal Body, especially the Head.—The only point I wish to note here, is the relation between the head and canalized pelvic floor so far as mere fitting is concerned. One factor in rotation depends on whether or not the head, for instance, passes through the pelvis easily or the reverse. In the production of malrotated head cases this is an important factor. Another way to put this is as follows. The question as to whether the occiput or sinciput will be deeper in the pelvis depends on whether the relation between the head and pelvis is such that the latter accommodates the suboccipito-bregmatic diameter or the suboccipito-frontal. The head is here considered as made up of the occiput and sinciput—the one begins behind, the other in front of, a coronal plane drawn at the posterior margin of the anterior fontanelle.

3. A DESCRIPTION OF THE MOVEMENT OF INTERNAL ROTATION IN THE VARIOUS PRESENTATIONS.

I briefly remark, first, that while the path of the head during rotation is a complex one, and is not exactly known, it is sufficient to describe it as a rotation of the head on an imaginary and varying vertical axis, occurring when the head is at a varying part of the true pelvis, usually below the level of the ischial spines, *i.e.*, when the leading part comes within the sphere of the sacral segment.

Internal rotation takes place in head, face, forehead, breech, and spontaneous expulsion cases. The clinical facts as to internal rotation in each of these is briefly as follows:—

HEAD.—In the L.O.A., the occiput, known by its small fontanelle, lies at the brim, opposite the left ilio-pectineal eminence, becomes driven down, and when it strikes the left lateral half of the sacral segment, *i.e.*, when asymmetric to the sacral segment, is rotated through quarter of a circle to the front and right, so that it lies wholly

or partially within the pubic arch, and as it now lies symmetrically to the sacral segment, rotates no further (*a*, Fig. 1, Plate I.)

In the L.O.A. the occiput rotates to the front and right, i.e., the part deep in the pelvis, and first striking the left half of the sacral segment, becomes rotated to the right and front.

In the R.O.A. the occiput rotates to the front and left through quarter of a circle, i.e., in the R.O.A., the part deep in the pelvis, and first striking the right lateral half of the sacral segment, is rotated to the front and left (Fig. 1, *b*, Plate I.)

In the R.O.P. we may have two rotations—(*a*.) *The normal or long*, and (*b*.) *The abnormal or short*.

(*a*.) *The normal or long* takes place so that the occiput passes from the region of the right sacro-iliac synchondrosis to the pubic arch. Clinically, the occiput is felt leading, and the occiput fits the pelvis accurately—the head is never a small one. In the R.O.P., therefore, when the occiput leads and strikes the right half of the sacral segment before the sinciput touches the left lateral half, it is rotated through three-quarters of a circle to the front and left (Fig. 1, *c*, Plate I.)

(*b*.) *The abnormal or short rotation* has been greatly misunderstood, owing to erroneous terminology. In R.O.P. cases where the head is small, the sinciput is deep in the pelvis, consequently strikes the left half of the sacral segment first, and is rotated by it to the front and right through quarter of a circle. Thus the occiput lies in the hollow of the sacrum, and no further rotation of the head takes place, inasmuch as the head is now symmetrically placed to the whole sacral segment (Fig. 1, *g*, Plate I.) *The malrotated occipito-posterior cases are really rotations forward of the sinciput, occur in cases where the head is small, and can easily be predicted during labour by noticing the deep position of the sinciput, i.e., deep position of anterior fontanelle.* They are best termed, therefore, after rotation, sinciput to pubis cases.

I need not detail the L.O.P., as we have either (*a*.) long rotation of the occiput from left to right (Fig. 1, *f*, Plate I.), or (*b*.) short rotation of sinciput from right to left (Fig. 1, *d*, Plate I.)

The rotation of the shoulders is simple. In the L.O.A. the right shoulder is anterior, strikes the right half of the sacral segment first, and is rotated by the pressure on its anterior aspect, to the front and left, through quarter a circle. The head is therefore externally rotated to the left (restitution).

In the R.O.A. the left shoulder is anterior, and is, for reasons already given, rotated to the front and right, causing external rotation to the right (restitution).

In the L.O.P. and R.O.P. either shoulder may strike a lateral half of the sacral segment, and thus rotation of the shoulders may occur to right and front or left and front in each (v. Plate I., Fig. 1).

Internal Rotation in Face Cases.—We name face cases according to the position of the chin (mentum) as L.M.A., R.M.A., R.M.P.,

L.M.P. In these positions the rotations are in all respects equivalent to the occipital one, the chin taking the place of the occiput. Thus in the L.M.A., the chin makes the short rotation to the front and right; in the R.M.A., it makes the short rotation to the left and front.

In the R.M.P., however, the normal rotation is the long one from the right sacro-iliac synchondrosis to the pubis; but we occasionally get, not the chin rotating directly to the front, but the forehead making the short rotation to the front. In the L.M.P. we have exactly a similar process. In malrotated face cases, therefore, where the chin after rotation is found in the hollow of the sacrum, we have really a rotation of the forehead forward.

In *Breech Cases* internal rotation is always short, the nearer hip rotating through quarter of a circle to the front. The rotation of shoulder and head is easily known, if the general law, to be stated at the end, is kept in mind.

In *Forehead Cases* rotation usually takes place so that the face lies behind the pubis, after which the occiput passes over the perineum prior to the escape of the face and chin under the pubic arch. Forehead cases are so rare, however, that the particulars in each form of the mechanism is not known with certainty. In the most remarkable form of mechanism known as *Spontaneous Expulsion*, where the doubled-up foetus is driven down into the pelvis shoulder first, and the breech driven over the perineum, we usually get, prior to this delivery of the breech, a rotation of the deep shoulder, so that it passes from the region of the obturator foramen to lie under the pubic arch. A case of this will be presently narrated. Finally, I may note that in the rickety pelvis, where the concavity of the anterior pelvic wall is much diminished, we may get internal rotation occurring as in an occipital case in a normal pelvis; while in the kyphotic pelvis the right occipito-iliac position is common, "the occiput rarely turns forwards, deep transverse position is common, and posterior rotation not uncommon" (Champneys).

4. THE CAUSATION OF THE MOVEMENTS OF INTERNAL ROTATION, GENERAL AND PARTICULAR.

It will help greatly to clear up our ideas as to the causation of rotation if we first exclude certain alleged factors.

Firstly, The shape of the rotated part can have no essential place in determining this movement, inasmuch as the shoulder in the pelvis, with its soft and varying outline, is rotated in spontaneous expulsions just as the occiput is.

Parts of the foetus of the most variable shape and consistence, if in the pelvis, may become internally rotated. Occiput, sinciput, chin, forehead, buttock, shoulder, all varying from one another markedly in shape, size, and consistence, yet rotate when once they come within the range of a lateral part of the sacral segment.

Secondly, The shape of the inner surface of the bony pelvis is not of essential moment; or, to put it in another way, the contour of the normal inner aspect of the true pelvis is not necessary to rotation. Rotation may occur in the rickety pelvis where there is great loss of concavity in the anterior and posterior bony pelvic walls, and where the much vaunted inclined planes have lost most of their inclination; or in the kyphotic pelvis, where we have changes diametrically opposed to those of the rickety distortion.

The *presence* of the anterior concave pelvic wall, on which posterior pressure may slide the deep presenting part, more than its precise shape, is important. The great factor in producing rotation is the recoil of one lateral half of the sacral segment on the part first touching it. We have seen that the sacral segment in the canalized pelvic floor is fixed above to the posterior portion of the bony pelvic outlet (*viz.*, sacrum and coccyx, and as far forward as ischial tuberosities), while it is free below. It is thus driven back by the successive portions of the foetus, causing the well-known bulging of the perineum. Rotation ceases when the part in the pelvis is symmetrical to the sacral segment, *i.e.*, when the head has rotated so that the occiput is symmetrically placed to the sacral segment, we get no further rotation. This shows clearly that it is the deep engaging part first striking a lateral half of the sacral segment, driving it back and causing its recoil, along with the guiding influence of the corresponding pubic ramus, which is the essential cause of rotation.

The recoil of a lateral half of the pelvic floor is not in the conjugate, but as near as may be at right angles to the insertion of that half into the bony pelvic outlet, *i.e.*, more or less parallel to the corresponding half of the anterior segment of the bony pelvis or to the pubic ramus (Fig. 2, Plate II.) This leads the way to the additional factors. The initial Solayres' obliquity of the head, shortness of the anterior pelvic wall, excess of elastic tissue in the posterior bony wall, and therefore greater friction on, and retardation of, the part lying there—all help to bring about a deep anterior engaging portion which shall strike a lateral half of the sacral segment, and be pushed forward during the pain in a direction nearly parallel to the corresponding half of the anterior segment of the bony pelvis.

We may now apply these general principles to the various instances in which internal rotation occurs.

In the L.O.A. the occiput strikes the left lateral half of the sacral segment early, for the following reasons:—The head with Roederer's obliquity (*i.e.*, occiput deeper at brim than sinciput), and with an antero-posterior diameter in the right oblique of brim (Solayres' obliquity), has its occipital end placed vertically above the left lateral half of the sacral segment, and in consequence of the shortness of the anterior pelvic wall, strikes it early as compared with the sinciput. The latter not only has the longer posterior pelvic

wall to travel along (Fig. 1, *x*, Plate II.), but is also hindered by the friction of the greater thickness of tissue there. The left lateral segment, therefore, recoils early on the posterior aspect of the occiput in a direction parallel to the corresponding half of the anterior pelvic wall, *i.e.*, the occiput becomes rotated. The head now lies symmetrically to the sacral segment, with its right and left halves resting on the corresponding halves of the sacral segment, and, therefore, no further rotation takes place. In the R.O.A., for the same reasons, the occiput strikes the right lateral half of the sacral segment, and is rotated to the front and left.

In the R.O.P. we have two possibilities in rotation to explain. There may be first what is termed the normal or long rotation of the occiput inward and to the left, from the region below the sacro-iliac synchondrosis to the arch of the pubis; and the abnormal rotation, where the sinciput rotates from a point below the left ilio-pectineal eminence to the posterior aspect of the pubis, the so-called *posterior rotation of the occiput into the hollow of the sacrum*. This last descriptive term is a very bad one, and should never be used, as both rotations are to the front—the one of the occiput to the front and left (Fig. 1, *e*, Plate I.), the other of the sinciput to the front and right (Fig. 1, *g*, Plate I.) The difference in the rotation is due to the relation in point of size or fitting of the head to the pelvis. If the head so fits the pelvis, *i.e.*, is so large that a sub-occipito bregmatic diameter engages in the pelvis, then the occiput first strikes the right lateral half of the sacral segment, and is rotated to the front; whereas, if the head engages in the pelvis with the sinciput low, *i.e.*, if the pelvis can accommodate the occipito-frontal diameter, then the sinciput first strikes the left lateral half of the sacral segment, and is rotated to front and right.

The same reasoning holds for the L.O.P. Usually the occiput first strikes the left lateral half of the sacral segment, and is rotated to the front and right; or the sinciput, when the head is small, first strikes the right lateral half of the sacral segment, and is rotated to the front and left.

In face cases we have in the L.M.A. and R.M.A. the chin leading and being rotated as in the occipital cases. In the R.M.P. and L.M.P. the size of the head is again influential. If the so-called extension is well marked, then the chin first strikes its corresponding half of the sacral segment, and is rotated to the front. The forehead, however, may be deeper, and thus the forehead is rotated to the front, the chin passing indirectly into the hollow of the sacrum. This happens when the fronto-mental diameter is small. I need not go into detail in breech and shoulder cases. In breech cases the anterior hip is deep, the posterior always being retarded, and thus the anterior is always rotated to the front.

The following clinical cases have been noted by me as helping in the clearing up of this problem:—

CASE I.—R.O.P. at brim; forehead in pelvis, and face at pubic arch. Child's head small, and occiput retarded by posterior pelvic tissue.

CASE II.—Seen with Dr M'Call. Prolapse of funis; cervical canal admitted three fingers; ribs presenting. Chloroform being administered, I passed my hand into the vagina and fingers through cervical canal, pulled down a knee, and left the case. An hour afterwards the breech was in the pelvis and leg down; and hips right and left. The leg pulled on rotated to the front, because this part was first pulled against a lateral half of the sacral segment.

CASE III.—Seen with Dr Stanley. Six months' foetus lying transverse and impacted in pelvis. Shoulder to left and front, and arm protruding. First movement was one of rotation of shoulder to front, and then the body and breech were driven over perineum.

CASE IV.—Seen with Dr Darling. Mento-posterior, with forehead low, and chin ultimately found in hollow of sacrum.

All these cases illustrate the law as to internal rotation, which I now state as follows:—

1. Whatever part of the foetal head or trunk first strikes a lateral half or lateral part of the sacral segment is rotated internally to the front, and in the direction opposite to the lateral half or lateral part of the segment so acting.

2. No part of the foetus is ever rotated directly into the hollow of the sacrum. The passage of the occiput into the hollow of the sacrum in its so-called posterior rotation is exactly equivalent to the passage of the sinciput into the hollow of the sacrum in normal rotation, and should be excluded from descriptive terminology, as the latter is.

3. The direction of rotation may be predicted in any case by noting what part first strikes a lateral half of the sacral segment.

This view of rotation gives, in my opinion, a more coherent idea of all the rotations than those based on the shape of the head or pelvis only. I do not deny the influence of the shape of the internal surface of the pelvis, but I deny that it alone can determine rotation.

It seems to me, also, that we here see an explanation of the pelvic inclination, Roederer obliquity, Solayres obliquity, and general shape of head and body of foetus.

The pelvic inclination, as we term it, gives us, by its slope, a short anterior pelvic wall, and may be looked on as due to a slicing away, as it were, of part of the anterior pelvic wall. The kidney shape of the brim, such that the head there does not fully occupy it, and lies with the Solayres' obliquity, is necessary for the distension of the bladder during pregnancy; while the shape of the occiput, chin, neck, and sternum allows of Roederer's obliquity in head

cases, or extension in face cases, so as to make a part, either occiput or chin, usually lead during labour. The short anterior pelvic wall and oblique and deep position of a part of the fœtus (occiput, chin, breech) bring necessarily the fœtal part lying anteriorly at the brim early within the reach of a lateral portion of the sacral segment, *i.e.*, tend to cause its early rotation.

DESCRIPTION OF PLATES.

PLATE I., FIG. 1.—Diagram of Rotation in Head Cases as seen through brim of pelvis. The arrows inside give the direction and amount of Internal Rotation; those outside, the direction of External Rotation.

F¹⁻⁴ = first, second, third, or fourth in frequency of occurrence.

N¹⁻⁴ = first, second, third, or fourth of Naegele.

Thus L.O.A., N¹, F¹ = left occipito-anterior, first in frequency and first of Naegele.

Arrow *a* = amount and direction of Internal Rotation of occiput in L.O.A.

" *b* = " " " " " " in R.O.A.

" *c* = " " " " " " in R.O.P.

" *d* = " " " " " " of sinciput in L.O.P.

" *f* = " " " " " " of occiput in L.O.P.

" *g* = " " " " " " of sinciput in R.O.P.

" *g'* = External Rotation of occiput in L.O.A.

" *k* (double) = possible External Rotation of occiput in L.O.P.

" *m* = External Rotation in R.O.A.

" *n* (double) = possible External Rotation in R.O.P.

FIG. 2.—Diagram of Internal Rotation in Face Cases.

Arrow *h* = amount and direction of Internal Rotation of chin in L.M.A.

" *i* = " " " " " " in R.M.A.

" *j* = " " " " " " in R.M.P.

" *l* = " " " " " " of forehead in L.M.P.
(chin to hollow of sacrum).

" *k* = amount and direction of Internal Rotation of chin in L.M.P.

" *m* = " " " " " " of forehead in R.M.P.
(chin to hollow of sacrum).

R.M.P. = right mento-posterior.

PLATE II., FIG. 1.—Sagittal mesial section of Pelvis to show Sacral Segment. *x* and *y* = length of paths described by fœtal parts lying posteriorly and anteriorly in pelvis.

e = axial coronal section of pelvis showing sacral segment. The dotted line separates the two lateral divisions; the arrows show the approximate direction of recoil of each lateral half on deep presenting parts of fœtus.

LITERATURE.

Only necessary literature given.

HART, D. B.—*Structural Anatomy of Female Pelvic Floor*, Edinburgh, 1880. *Contributions to Topographical, etc., Anatomy*, Edinburgh, 1885.

LAHS.—*Die Theorie der Geburt*.¹

¹ While admitting fully the value of Lahs' work and Professor Simpson's extension of it, I wish to urge that the special wedge shape of the head is not necessary to flexion or rotation. Lahs evidently believes that the priority in movement of the occiput in flexion and rotation is due to its

LEISHMAN.—*The Mechanism of Parturition*. London, Churchill, 1864.

LUSK.—*Science and Art of Midwifery*, London, 1882. Lusk's description of the cause of rotation is suggestive. He says "it is always the most dependent portion of the presenting part which rotates to the front."

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SIMPSON, A. R.—*Contributions to Obstetrics and Gynecology*, Edinburgh, 1880 (article, "Head Flexion in Labour").

STEPHENSON.—"On the Mechanism of Labour," *Lond. Obstet. Jour.*, vi. p. 401.

WEST, UVEDALE. — "Cranial Presentations and Cranial Positions," *Glasg. Med. Journal*, 1875. This is a valuable article, and contains the following interesting statement in regard to malrotated occipito-posterior cases:—"The cases which terminate with the face or forehead at the pubes are originally positions in which the uterine efforts are so perversely directed that the forehead gets down into the pelvis during the first stage" (p. 323, conclusion 11). . . . "The anterior or frontal end of the head will frequently pass first to the floor of the pelvis and then come forward to the arch of the pubes" (p. 323, part of conclusion 15).

VI. — CASE OF TOTAL EXCISION OF TONGUE (FOR EPI- THELIOMA) THROUGH THE DIGASTRIC TRIANGLE, WITH PRELIMINARY LARYNGOTOMY.

By FRANCIS WILLIAM GRANT, M.D. (Edin.), C.M., B.Sc. (Public Health); formerly Senior Demonstrator of Practical Physiology in the University of Edinburgh.

I TRUST the following case may be deemed worthy of insertion in your valuable Journal, partly on account of the good results following the operation, and partly from the fact that the mode of

special shape, but this seems to me more satisfactorily explained as I have already done. Professor Simpson, as we know, has measured the angles in Braune's plate of a parturient woman, and found that the occiput is subtended by a less angle than the sinciput. This would seem conclusive, but we get the same angles at shoulder and body in Chiari's plate of an impacted cross birth. Then, again, Lahs explains the passage of the occiput over the perineum prior to the sinciput coming under the pubic arch by this special angle again. But in spontaneous expulsion the breech is driven over the perineum in the same way, showing, I think, that this happens because the occiput or trunk is so driven through because it lies over the sacral segment which yields back. I mean by this that it is the sacral segment which yields back to let the occiput or breech pass, and not any special shape of the occiput which favours it.

I hold, therefore, that Lahs has not brought forward sufficient evidence as yet in support of his theory. In saying this I merely condemn my own hasty acceptance of it in a paper read before this Society in 1879.

excision adopted, though constantly practised in some of the Continental schools, is rarely, if ever, followed by Edinburgh surgeons.

W. W., æt. 56 years, married, a carter by occupation, and residing in the village of Lhanbryde, near Elgin, came under my notice on the 2nd day of April 1885, suffering from a painful growth on the tongue.

History.—Patient states that about five months previous to the day on which he consulted me, he experienced a burning sensation on the left side of the tongue, which continued with unabated severity till about five weeks later, when, on examining the left edge of the organ about its middle, he was aware for the first time of the existence of what he called a “flattened pimple about the size of a split pea.” This “pimple” steadily increased in size, and from the discomfort it produced during mastication, and the shooting pain constantly experienced in the part, he was led to seek medical advice. He affirms that for the last thirty years he was in the habit of smoking three ounces of Irish roll tobacco per week, and expressed satisfaction that for the last three years he was able to enjoy the weed, through the *same* short, well-seasoned cutty pipe. I also learned from him that it was his invariable custom to keep the pipe in the left side of the mouth. There is no history of syphilis, and his family history appears good.

On examination I found a well-defined oval tumour, flattened on the surface, and over $\frac{3}{4}$ of an inch long, situated partly on the dorsum and partly on the left edge of the tongue, about the junction of its middle and posterior thirds. It was hard and unyielding to the touch. There was also noted a suspicious thickening along a corresponding portion of the median raphe. There was marked induration in the mucosa of the floor of the mouth, underneath the lingual growth. Three enlarged and hard lymphatic glands were felt underneath the ramus of the jaw on the same (left) side, and another, much larger, over the angle of the jaw. Otherwise the man appeared in good health, but complained of great debility, which he believes to be due to his many sleepless nights, and inability to eat. From the unmistakable nature of the lingual growth, and the existence of neighbouring indurated glands, I resolved, after consultation with Drs Ross, Duff, Whyte, and Adam of Elgin, to remove the entire organ and affected glands through the same opening, by the inframaxillary operation, devised and practised by Kocher.¹

Operation, 5th April 1885.—The patient being anæsthetized, laryngotomy was performed, and a tube placed in the larynx in the usual way, through which the inhalation of chloroform was continued during the rest of the operation. The tongue was then transfixed by strong whipcord at its tip, and after dividing the frænum some little distance, to enable the tongue to be brought well forward, the pharynx was thoroughly plugged with a large car-

¹ *Deutsch. Zeitsch. für Chirurg.*, Bd. xiii., page 147 (1880).

bolized sponge, to which whipcord was also attached. An incision was next made, commencing a little below the tip of the left ear, down the anterior border of the sterno-mastoid as low as the great cornua of the hyoid bone, then forwards to the body of the latter, and from this upwards along the anterior belly of the digastric. The flap of skin thus formed was reflected on to the cheek, and four indurated lymphatic glands carefully removed from before backwards. The superficial portion of the left submaxillary gland, from an unnatural consistence in its structure, was mistaken for a cancerous lymphatic gland, and was thus removed along with its deep portion. After ligature of the facial and lingual arteries—the latter just before passing under the hyoglossus muscle—the floor of the mouth was opened into, and the diseased mucosa at the same time removed. A loop of the ecraseur wire was introduced into the buccal cavity through the digastric wound, and made to encircle the tongue as far back as possible, and then adjusted to the ecraseur. The organ was by this gradually constricted, about five minutes being allowed to sever the tissue. As the pulse became irregular, I rather hastened the last two turns of the ecraseur, the result being that the stump bled freely for a short time. This was, however, soon controlled by plugging the mouth and the digastric opening with sponges, and did not again recur. After the mouth and pharynx were rinsed out with weak carbolic lotion, the pharyngeal plug was withdrawn, and the submental wound stitched, except for an inch or so about its middle, which was left open for drainage purposes.

After-Treatment.—For the first twenty-four hours I fed the patient with enemata of port wine, beef-tea, and cream. From the second to the end of the sixth day the nutriment was introduced into the stomach by means of a stomach syphon tube. After the seventh day the patient was allowed to swallow liquids and semisolids. The tube in the larynx was retained until the tenth day, when it was removed, and the wound allowed to granulate over. The digastric wound was twice daily syringed with a weak lotion of Condyl's fluid and chlorinated soda. The stump was twice daily dusted over with iodoform. By these means there was hardly any fetor of breath perceptible, and the temperature never rose above 99°·6 (evening of second day after operation). Four weeks after the operation I lost sight of the patient until the end of November of last year, and again on the 17th March of this year (1886), when he came, at my written request, to show himself.

Present Condition.—He is now much improved in general health—his complexion ruddy, and face cheerful (a marked contrast to his former state). He eats well, and has gained over one stone in weight since the operation, over eleven months ago. The lingual stump presents the same healthy appearance as the buccal mucosa. Not only the stump, but the floor of the mouth and both digastric triangles are quite free from induration; in fact, there is nothing

in his condition to indicate that the disease is likely to recur. His power of speech is wonderfully good, and with little difficulty he is understood by strangers. All that he complains of is the trouble which the saliva trickling from the mouth gives him.

Remarks.—It is not for me to say anything anent the comparative advantages of one operation over another, therefore I will only briefly note one or two points which I think of interest in this case. *1st*, By entering the floor of the mouth through the digastric triangle I was enabled, through the same incision, to remove the diseased organ and the affected glands, and afterwards to leave a good channel for drainage purposes; *2nd*, By the use of iodoform to the stump, and Condry's fluid and chlorinated soda through the wound, and by introducing pure air into the lungs through the laryngeal opening for ten days after the operation, the risks of pyrexia or pneumonia from septic inhalation were minimized; *3rd*, By the use of enemata and stomach tube the man received more nourishment than he could otherwise have got, and without interfering with the healing stump; *4th*, Had it not been for the preliminary plugging of pharynx and laryngotomy, the hæmorrhage, which was so profuse at the end of the operation, would probably have proved fatal, seeing the patient was recumbent and deeply anæsthetized; and, *5th*, By working the ecraseur from the digastric triangle at the same time that the tongue is forcibly drawn out of the mouth by means of the whipcord attached to it, the risk of the wire slipping forwards was greatly lessened.

VII.—THE UNITY OR DUALITY OF SYPHILIS HISTORICALLY CONSIDERED.

By J. L. MILTON.

(From a Paper read before the Willan Society.)

NEITHER the sentence of death passed on duality at a well-remembered meeting of the Pathological Society of London, nor the experiments of the German physiologists, have availed to convince some men that the doctrine of two poisons must be given up, or even induced them to think that there is more reason now to doubt its vitality than there was ten years ago. Proof enough of this lies scattered through the various journals. In one of his last contributions to the literature of the branch which he so long adorned, Dr Bumstead says that such a statement as that unity is dead is "a fraud upon medical history, and a source of confusion to the student;" and even Professor Auspitz, one of the most thoroughgoing uncompromising defenders of unity, expresses himself in his latest communication on the subject rather less forcibly, it appears to me, than he did.

This justly famous pathologist asks the question, "Where are we to-day in respect to syphilis?" To which I should say the answer must be, "As far as concerns the unity or duality of the disease pretty much where we were some 350 years ago, when the reign of confusion began, and where we are likely to be 350 years hence, unless some more rigorous mode of investigation is adopted than I have ever had the good fortune to see put in force."

I propose, therefore, without entering upon any pathological questions whatever, to take what is, after all, indispensably necessary for a due examination of them, the historical part of the subject, and ask my readers, after weighing what I have put forward, to say whether they believe or do not believe that soft non-infecting sores were seen and described long before the epoch usually assigned to the great outbreak of syphilis? Secondly, whether they can point to a single authentic description of constitutional syphilis prior to the fifteenth century? And, lastly, whether, after having satisfactorily disposed of these two points, they feel inclined to affirm or to deny that syphilis, whatever be the decision now as to its simple or compound nature, was once double? The decision on these heads, be it which it may, will conduct us by a threefold path to a goal in which they naturally end,—to the first great crucial question which demands solution at the hands of the believer in unity, a question far too long overlooked, and that is whether a disease, once purely local and not including hard sore, not followed by secondary affection, all at once expanded into a malady which taints every organ and tissue of the frame, a malady from that date usually ushered in by hard sore; or if two diseases, hitherto totally distinct, suddenly became so fused together that they have ever since constituted one complaint; for, however impossible such a thing may appear, one of the two must have happened. It seems to me that it will be much better to try to get at a definite expression of opinion on this point than to make any attempt to embrace too wide a field. The bane of medical investigation has been, that only too often it did not sufficiently narrow the subject actually under discussion; that it admitted, almost encouraged, far too much laxity of expression, too much in the way of assumption; that it sacrificed to authority what ought to have been yielded only to argument; that it fostered the habit of allowing conviction to take the place of demonstration, and only too often elevated a high-sounding theory above fact. But if ever medicine is to attain to that accuracy which distinguishes the exact sciences, it can only be by means of that strict unsparing examination which these branches of study demand and will have. Consequently, if I appear to have dwelt with unnecessary minuteness on such a purely introductory part of the subject, I must plead guilty, but ask leave to justify doing so on the ground that I wish first of all to see the points just mooted thoroughly threshed out and sifted, so that the way may be made clear and straight, step by step.

It is generally said that to Bassereau we owe the remark that the earliest writers on syphilis, after the great outbreak at the close of the fifteenth century, drew a clear and broad distinction between the sores on the penis resulting from connexion, so often described in foregoing times, and the new disease which had so suddenly broken in upon them, and that it was not the first observers, the men who actually saw syphilis make its great irruption, who confounded the two forms of it. But in point of fact the same opinion was set forth long before by Hensler, who had perhaps more thoroughly mastered the views of the old writers on this subject than any other man. "It is certain," he says, "that a lesion like the sore of impure connexion, and arising from the same impure cause, existed before syphilis." He might have put the case perhaps more strongly; still, even in this shape, unless this means preaching duality, I am at a loss to know what it does mean.

I leave on one side all such questions as whether this lesion, or the old form of syphilis as it might be called, existed from time immemorial in India and China, as maintained by Klein, Nelson, and others. Such evidence would only obscure the argument. I pass by, too, not because it is untrustworthy but because it is superfluous, the description of venereal sores given by Celsus, though I should consider it of itself sufficient to settle the question; and Dr Joseph Adams, the contemporary and rival in fame of Home and Willan, whose work on morbid poisons attests the depth and accuracy of his judgment, points out that we have here every form of sore typified except the true Hunterian chancre, and that the sloughing phagedæna, nigrities serpens of Celsus, is the same disease as that described by Abernethy. I consider, too, the evidence of the votive tablets in an old Pagan temple near the Seine, found and described by M. Becquerel, equally decisive; it seems to me that it would never have entered the head of any one to sketch such things unless he had had the reality before him. But I will suppose that the reader may reject such testimony as savouring rather too much of antiquity, perhaps of obscurity—that he may prefer to demand the evidence of much more modern times—and I therefore proceed to deal with this.

Between the year 1270 and the great outbreak of syphilis in 1494, we find at least seven authors mentioning such symptoms as "ulcers of the yard," "ulcers from pustules of the yard," "mortification of the substance of the yard," "imposthume of the groin," all attributed to "lying with a foul woman," "a foul, nasty, or cancerous woman," "a woman having an ulcer of the womb," and so on. We may read in at least one author of sores piercing between the "skin and flesh" of the penis, the very feature which Judd gives as characteristic of the black lion, "burrowing under the foreskin and dissecting between the integument and the body of the penis." About the close of the fourteenth and early part of the fifteenth century several persons are spoken of as suffering under phagedæna

arising from connexion. And, therefore, seeing that burrowing sores brought on by illicit intercourse, phagedæna, and bubo are in the present day constant and natural adjuncts of chancroid, the conclusion that soft sore existed in the times just referred to seems to me as justly founded as any conclusion in pathology can well be.

Attempts have been made to explain away the appearance of such sores. It has been asserted that they were merely due to irritation set up by connexion, an irritation fostered by dirt and debauchery on the part of the recipient. But I should say that there are plenty of patients to be seen every day in our large hospitals fully as dissipated and filthy as ever were the people of the thirteenth and fourteenth centuries. Mr Holmes Coote learned from some of the outcast girls, who used in his time to hang about the dark arches of the Adelphi, that it was quite a common thing for one of them to have connexion in a single night with eighteen or nineteen wretched profligate creatures like themselves. Yet though he more than expected to find syphilis generated in this way, he never could, with all the pains he took, trace it to excesses of this kind. For a long time I had unusual opportunities of seeing the out-patients in two large hospitals, and I can safely say that I saw very little indeed of phagedæna and bubo accompanying sores on the penis which did not in the end turn out to be syphilis. One author has denied that such lesions were sores at all, arguing that because the old writers believed and taught that gonorrhœa, the source of which they could not see, was due to an ulcer, therefore they must be equally mistaken about these sores—the sanies which they supposed to be issuing from an ulcer being only the product of a balanitis. This seems to me strange reasoning, and rather like fighting for a lost cause. It is most probable that men first saw pus coming from an ulcer, and then very naturally inferred that there must be an ulcer in a canal from which they saw pus coming; also it is most probable that sores piercing “between the skin and flesh” were ulcers and not simply balanitis. Besides, the old writers recognised a like sore in the female giving the infection; they mention the signs by which it could be detected in her through the medium of the sight and touch, *visui et tactui manifesta* are the words employed, and according to one author such a sore would be revealed by the speculum when it could not be made out by the naked eye.

During all this time we have nothing of hard sore; I have entirely failed to meet with any description of such a lesion. Auspitz, indeed, says he has shown that it was described at a time when it is stated by the dualists to have been non-existent. I would always rather be on the same side with this distinguished teacher, but here I must range myself in opposition to him. The evidence on which he relies is a passage in Valescus of Tarentum, who says “he has known some patients die because they did

not obtain medical assistance in time. The penis was entirely surrounded with a cancerous ulcer having a certain degree of hardness, and also round like a turnip, and the patient was already livid and half dead." But this is a description of strangulation from paraphymosis neglected and mismanaged; it has nothing in common with hard sore, little even with chancrous erosion, while it may be seen almost daily under a milder form in our hospitals as a result of chancroid. The earliest writers on the morbus gallicus never allude to such a state. If there had been any necessary or even frequent connexion between such cases and true syphilis, shrewd observers like Leonicensus, experienced army surgeons like John de Vigo, who wrote so fully and ably on this disease, would almost certainly have mentioned it, would have recognised the new malady in the description given by Valescus, with which they, the former of them especially, were sure to be familiar, seeing that he was a well-known author. Even supposing it had been hard sore, the historical evidence would still have been as strong as ever in support of the view I have ventured to recommend. While numerous accounts attest the almost constant occurrence of local sore during more than two centuries, all that the industry of a man of such extraordinary acquirements as Auspitz can bring forward on his own side is one solitary case of very dubious nature. Auspitz also maintains that a passage from the same author must be construed as indicating a knowledge of chancre in the urethra, a lesion nearly always, if not always, followed by constitutional infection. With regret I must again range myself on the opposite side; the passage is not very clear, but it is to this effect, "The ulcers and pustules which form on the penis are sometimes in the outer skin, sometimes as far as possible within it," and my contention is that Valescus meant by the latter part of the sentence, not the canal of the urethra, but the reflexion of the prepuce. The reader can, however, see the original words quoted in Auspitz's work on *Syphilitic Contagion*, page 26, and judge for himself.

The authors spoken of as describing soft sore in the thirteenth and fourteenth centuries never allude to this as a constitutional affection at all, and one of them distinctly says it is purely local. But when syphilis first began to alarm Europe, the fact that the disease generally arises from chancre on the penis only dawned very slowly on the minds of even the best observers; in the case of the sores described by their predecessors we find no trace of such obscurity. They never doubted about the matter; so far as it went their pathology was certainly concise and most probably correct. Their treatment was suited to their pathology; they employed only topical means, and never dreaded secondary disease from restricting themselves to such simple measures. Professor Auspitz, indeed, quotes from Gerardus de Berry, an author of the thirteenth century, whose work I have not seen, testimony which he looks

upon as conclusive, that at that early date secondary disease was ascribed to sores derived from impure connexion. I refrain from expressing any opinion as to whether the evidence will commend itself unreservedly as conclusive to his readers, but in reality it amounts to no more than the author he quotes from saying, that "the penis becomes infected, and that the health sometimes suffers in consequence." Why, the same thing was habitually said for ages about gonorrhoea, and there is not a single symptom, not a sign of secondary disease mentioned; in short, I cannot understand how such a simple phrase of four words is to be interpreted as meaning any distinct disease whatever, especially one like syphilis, which men described forcibly enough at a time when we know that they really did see it—that is to say, at the close of the fifteenth and beginning of the sixteenth centuries. But granting—and with all deference to Professor Auspitz it is granting a great deal—that true syphilis is meant in the passage from De Berry, it would merely show that this disease was occasionally seen at the early date mentioned, only to disappear again for centuries, as certainly no other writer at all near that time was acquainted with any constitutional disease arising from sores on the genitals; nor can the syphilis which De Berry is supposed to describe be connected, by any stretch of argument, with the primary lesions spoken of by Lanfranc, Gordon, Arnald of Villanova, and others. That a few scattered symptoms of constitutional disease, such as boonhaw (node), suspicious ulcerations of the face and throat, are mentioned now and then by some writers of the dark or mediæval ages is quite true; but these distant allusions only appear to fade away again for a long series of years; they reveal no consciousness on the part of the writer that he was dealing with a severe individualized constitutional disease; whereas the first writers on true syphilis have given us the portrait of a malady in which we distinctly recognise the syphilis of our day, and the unbroken descent of which down to our time I need not stop to point out.

Until we come to the time when men were so startled by the great outbreak of syphilis, we never find writers branding a disease derived from sexual intercourse with such epithets as were speedily bestowed upon syphilis. No doubt a great deal of this was exaggeration—as, for instance, in the case of Grünbeck, so often quoted as an authority, whose spasmodic style of writing, with his allegorical description of the spectre of humanity and pedantic comparison of the encroachments of the disease to those of a hostile force making its way into a citadel, is not calculated to gain our confidence, interesting as his work is. But behind all objections of this kind lies the great fact that we never in earlier times find a trace of this exaggeration itself, that not even leprosy is spoken of in this manner. And when we see one author after another impregnated with such views of syphilis, and even a man

of so moderate and practical a bent of mind as de Vigo speaking of it as "thys shamefull and abhominable dysease," and all this without the least allusion to its having been known in the days of Lanfranc and Valescus, we may be sure that to them, at least, it was new as well as abhorrent.

We must not omit from consideration the extraordinary ignorance on the part of men, so well able to judge as the first authors on syphilis were, of the newest and, I suppose, still predominant theory about chancroid, namely, its origin from the inflammatory products of chancre. Such an idea as that of the ulcers on the parts of generation, described by a long succession of authorities, being simply a local form of the morbus gallicus or pudendagra, never occurred to men like Schellig and Pinctor, like Marcellus Cumanus, present with the Venetian and Milanese army in 1495, or John de Vigo who, though writing later, had evidently mingled with the actors on the scene, and might be fairly spoken of as one of them. Misled by no specious theories, these men naturally treated as distinct two diseases which, viewed by the light of history and judged by their visible signs, were entirely different in their origin, course, and result. They were, perhaps, not quite so familiar with chancroid as Bassereau and Chaballier would have us believe; but so far as they were acquainted with it, they appear to have seen it from the same point of view as their predecessors. For all practical purposes the old soft sores, the ulcers between the foreskin and the glans—*puncturæ intra corium et carnem*—were in their eyes as distinct from the new pestilence as one disease could be from another. With the arrival of the latter we encounter a totally different pathology and practice. The search for the cause of the morbus gallicus in the baneful influence of planetary conjunctions or atmospheric agencies upon the humours, especially the bilious, the immediate recognition of the wide-spread infection of the system, and the necessity for general treatment, particularly by means of mercury, appear with the general appearance of syphilis and no sooner. In the eyes, then, of the authors who flourished at the close of the fifteenth century and the opening of the sixteenth, syphilis was essentially twofold; and the least reflecting person can understand that they were not likely to descant upon either the distinction or connexion between two affections which as yet no one had ever dreamed of confounding, or upon a genesis of chancroid, the discovery of which was reserved for modern ingenuity.

I believe disease to be as unchangeable in its nature as the elements around us. It gains or loses hold upon the human frame; its severity is lessened or aggravated, mostly by causes which we cannot fathom, but to the last it wears the same radical characteristic features as at first. The scarlatina of this century seems to be more fatal than that of Sydenham's day, but it is still simply scarlatina; leprosy is less formidable in Iowa and Minnesota

than in Norway and Guiana, but in no other respect does it change one jot. The sweating sickness of 1517 was more fatal and rapid than that of 1485, but, I suppose, no one ever doubted their identity in nature. The cholera of 1854 was more deadly than that of 1849, yet cholera has not altered in one essential characteristic since it first issued from the jungle. Many other instances of these mutations of severity might be cited, but not one of them points to a fundamental change like that which we must admit if we are to accept the unity of syphilis.

It seems to be in the order of things, that old opinions, however well founded, must, for some reason or other, yield to those which possess the charm of novelty—that the generation of to-day must overlook or reject something which a previous age had received with general assent. Thus it proved with respect to syphilis. By the time that the pathology of the disease had begun to assume completeness of outline under the hands of Paracelsus, 1527, and Fernelius, 1556,—that the dependence of the constitutional symptoms upon a specific primary affection, itself produced by a poison acting at the point of contact, had come to be recognised, although still encumbered with much that was erroneous as regards the nature of this lesion,—the old historical landmarks had almost been lost sight of, and very soon after the time of even the first of these two writers the epoch of confusion had fairly set in. As this is the turning-point in the history of our subject, I propose to examine it rather fully.

MM. Bassereau and Chabaliere date the change in men's opinions as to the quality of syphilis from the account of this disease given in 1508 by Vella. If so, his readers were very easily convinced, for, I think, a piece of worse reasoning could scarcely be found; and if he can be supposed to have proved anything, it is that he has utterly confuted himself. He saw the disease both before and after the outbreak of the morbus gallicus, and expresses his surprise at finding it from that date followed by constitutional symptoms, because the initial lesions seemed to him identical. "The way in which these patients are infected," he says, "is the same as that in which the virile members were infected before this disease (the morbus gallicus) existed—that is to say, by the act of coition, by the same organ, and, so far as the power of sight enables one to judge, by the same pustules. For which reason, those skilled in the art (of medicine) not being able to see any distinction between the one and the other, proceeded in the old way with the treatment of the new disease." Unless this means that chancre not only existed, but was fairly common before the assumed date of the coming of syphilis, I am utterly at a loss to know what it does mean. If the fact of an author, writing on the spot, treating one disease as new and never alluding to the other in any such way, be not evidence of the pre-existence of that other, pray, of what is it evidence? what does it go to prove? Here, too, we see scattered

to the winds all the arguing about the soft sores of the thirteenth and fourteenth centuries being common ulcers, for Vella tells us that he could not distinguish them from sores which were clearly the first lesions of syphilis. Therefore, if the older sores were not chancroids, chancroid has never existed at all, and must be treated as a myth. Of course, Vella's mistake was that he converted an occasional resemblance into a rule. The occasional resemblance we know to be an established fact, seeing that some of the best observers, even M. Ricord himself, have not hesitated to confess that they could not always make out the nature of a sore from its visible signs.

Here, then, we see that the confusion between the two forms of syphilis, soon afterwards to be openly taught, is fairly hinted at on authority, and had already begun to disturb men's opinions; but for all this, I consider that both Bassereau and Chabaliere have erred in ascribing the beginning of the belief in the unity of syphilis to the weight of any arguments employed by Vella, for I am inclined to think that he never had influence enough, either with his contemporaries or with later writers, to gain currency for his views. He seems to have scarcely ever been mentioned by either class of authors, and his practical knowledge of the disease to have been in rather a crude state, for he was evidently not acquainted with the Hunterian sore, common, according to what de Vigo says, after the invasion of Italy, seeing that he could not very well have mistaken it in a developed state for one of the old soft chancres. It is not true, too, that those skilled in medicine treated both kinds of primary sore—that is to say, the lesion seen before, and that first noticed after, 1494—in the same way; not a passage can be produced from any standard writer on syphilis to such effect. The purely local method pursued by the practitioners of the fourteenth and fifteenth centuries is as distinct as can well be from the constitutional means employed almost at the first appearance of syphilis, especially as regards the use of mercury, recommended as early as 1495; and for my part I cannot understand any person maintaining the identity of the two systems. Vella's evidence, therefore, in support of the unity of syphilis, which he is trying to enforce, must, I contend, be rejected, and, as well as I can make out, was rejected for long after.

It was not even a novelty on his part to trace a likeness between the old sores and the primary lesions of syphilis. Six years previously Almenar had described the new disease as beginning with a corrosion on the penis like the caroli. But I must totally dissent from the sweeping statement of a modern writer, that when syphilis first broke out the authors of that day, Marcellus of Como, Leoniceus, Beniveni, Cataneus, de Vigo, Vella, and even Fracastoro, found no difference between the old caroli pudendorum and the pus sores on the genitals, which they described as an accidental symptom preceding syphilis! Fracastoro is not entitled

to a hearing, as he could only have learned about the matter from others. With Vella I have already dealt, and de Vigo must be eliminated, for his classical description of hard sore is the earliest thing of its kind; no such portrait of Hunter's chancre exists before his time. As regards three of the first four names I can say nothing; the author in question does not quote his references, and I have searched in vain for a single passage to which such an interpretation could be given. The evidence of Cumanus I must read the other way, for though he saw the two diseases side by side, he does not seem to have thought of confounding them; and his description of sores the size of a millet seed, whether it refer to itch or to follicular chancre, would be as difficult to find in any author before his era as would that by de Vigo. But, indeed, the question is one, not of resemblance, for that with some reservation may be admitted, but of identity. On the latter point the evidence of the earlier writers after 1494 is decisive; they saw no identity. And as I understand Vella, some of his contemporaries were anything but convinced that the two affections were one and the same, for in substance they were asking the question, "Why, if this be old, did we never see such results till now?" And the reason why is, according to him, a new state of the atmosphere.

The mystification was not long to be delayed, and the task fell into the hands of two of the most incompetent and blundering observers of that or any other age, Massa and Bragsavolus; the first effectually began, and the other thoroughly completed the work of confusion. Massa, a person of narrow, superstitious views, who in an age of inquiry had not advanced a step beyond the crude opinions of the earliest observers, added, 1532, chancrous bubo to the body corporate. Bragsavolus, 1551, joined to the two forms of syphilis, now fused into one, gonorrhoea, recognised as a local complaint at least as far back as the time of John Arden. They carried the day. Though they were opposed by men like Fracastoro, Scaliger, and Fallopius, the tidal wave of obscurity which they had raised buried for a time at least all dissent, and has made itself felt often enough in the present century. Such men, having adopted an idea, seem incapable of any further impression from either arguments or facts, and adhere to the first conviction in much the same way as the bivalve clings to the rock on which it has anchored itself.

It will be unnecessary to reproduce the opinions of Fernelius, Fallopius, and others. They effected no particular change in the general belief. It will suffice to say that the reign of chaos, having fairly set in, went on with unvarying monotony, reaching, perhaps, its lowest depths in the pages of Sydenham, who wrote, I should say, the worst description of syphilis in the whole world,—the catastrophe in which, according to him, this disease naturally closes being a picture utterly untrue to nature, though quite in keeping with the rest of his account. "The limbs," he says, "rot

away piecemeal, and the mangled carcase, having at length grown hateful to the living, is buried in the earth." The views of Turner and others who followed Sydenham need not detain us, as they go for nothing in this question, the first real step towards the solution of which was reserved for Hunter, with whom history ends and pathology begins.

VIII.—REPORT OF CASES ADMITTED TO WARDS X., XI., AND XII., ROYAL INFIRMARY, EDINBURGH, UNDER THE CARE OF MR BELL, FOR SIX MONTHS ENDING 31ST OCTOBER 1885.

Reported by W. SCOTT LANG.

THE number of cases admitted, exclusive of those in the Wards at 1st May 1885, was as follows:—

To Ward No. X.,	139 males.
XI.,	110 females.
XII.,	104 males.

Total, 353

There were 14 deaths—equal to a mortality of rather under 4 per cent.

The principal cases may be classified as follows:—

Injuries.

	No.	Recovered.	Died.
Fractured Limbs (simple)	20	20	0
" (compound)	16	16	0
Bruises and Sprains	17	17	0
Concussion of Brain	10	10	0
Wounds { Lacerated 16	21	21	0
{ Penetrating 2			
{ Gunshot 3			
Dislocations of Shoulder	2	2	0
Fractured Pelvis	2	1	1
Compound Dislocation of Ankle	1	0	1
Fractured Spine and Sternum	1	0	1
Fractures of Skull	5	4	1
	95	91	4

The fatal cases of fractured pelvis, fractured skull, and fracture of spine and sternum, were all caused by falls from great heights, and in each case death occurred within a few hours of admission.

In the case of fractured pelvis, it was found at the autopsy that the head of the femur had been driven through the acetabulum, and was lying in the pelvic cavity. The bladder was ruptured, and death occurred in a few hours from shock.

The case of compound dislocation of ankle occurred in a very stout man, aged 63. There was no rise of temperature, and he was apparently doing well; but he died suddenly on the fifteenth day, from failure of a fatty heart and coagulation of blood in the great vessels.

Surgical Diseases, etc.

	No.	Cured or Relieved.	Died.
Diseases of Joints	45	45	0
Necrosis and Caries of Bone	26	25	1
Tumours, Innocent	32	32	0
„ Malignant	15	14	1
Hæmorrhoids	7	7	0
Ulcers	17	17	0
Cystitis and Stricture of Urethra	14	13	1
Calculus in Bladder	2	1	1
Ununited Fractures	3	3	0
Spinal Disease	5	5	0
Hernia	2	1	1
„ Strangulated	2	1	1
Empyæma	2	2	0
Gangrene of Extremities	2	1	1
Abscesses (Various)	25	24	1
Bursitis	6	6	0
Phlebitis	2	2	0
Carbuncle	1	1	0
Perityphlitis	1	1	0
Painful Stumps	4	4	0
Spina Bifida	1	1	0
Œsophageal Stricture	2	1	1
Strumous Testicle	2	2	0
Harelip and Cleft Palate	5	5	0
Hydrocele	5	5	0
Talipes	3	3	0
Diphtheria	1	0	1
Various	26	26	0
	258	248	10

The number of operations was 227.

Amputations and Excisions of Joints	34
Other Operations	193
	227

Amputations of Limbs.

	No.	Recovered.	Died.
At Ankle	4	4	0
In Leg	1	1	0
In Thigh (Carden's)	6 (primary)	6	0
„ Upper third	1 (primary)	0	1
At Wrist	2 (primary)	2	0
Upper Extremity and Scapula	1	1	0
	15	14	1

Excisions of Joints.

	No.	Recovered.	Died.
Elbow	6	6	0
Shoulder	1	1	0
Wrist	1	1	0
	8	8	0

Amputations of Fingers, Toes, etc., ... 11

The only fatal case was that of a man aged 62, who had his leg

and thigh terribly lacerated by being run over by a tramway car. He was drunk at the time, and had been in a chronic state of alcoholism for two years before the accident. Amputation was very rapidly performed just below the lesser trochanter, but the patient survived only twenty hours.

The case of abscission of scapula and removal of upper extremity, along with outer two-thirds of clavicle, has been already published, in order to notify a new mode of restraining hæmorrhage, which was successfully adopted. Patient was admitted on 11th May. The operation was performed on 14th May, and he went home cured on 22nd June.

Other Operations.

Strangulated Hernia, 2; 1 recovered, 1 died. Both were femoral, and occurred in women. The cause of death in the fatal case was not quite clear, even at the autopsy. There was no peritonitis, but the heart and liver were found fatty, and death was attributed to shock.

Amputation of Penis for Epithelioma.—Two cases. Both made excellent recoveries. In these cases the corpora cavernosa were each stitched, so as to draw their superior and inferior margins together, and prevent oozing from the cut surfaces. The result was highly satisfactory.

Hydrocele.—Five cases; all cured. In three of these cases the ordinary iodine injection was used. In the two others the tunica vaginalis was laid open, and stitched to the skin of the scrotum. A wide but short drainage-tube was then introduced, and held in its place by a stitch, so as to allow the fluid to escape readily as it collected. This method seems preferable to that of injecting iodine, especially in old chronic cases. The cure is more complete and certain, and the pain is not so severe, and does not last so long. No chloroform was administered.

Tracheotomy.—One case, for diphtheria. The dyspnoea was urgent, but the disease extended downwards after the operation, and the membrane appeared at the wound. Death occurred suddenly on the seventh day from paralysis of heart.

Removal of Tumours.

Excision of Breast and Axillary Glands for	No.	Recovered.	Died.
Malignant Tumour	10	10	0
Other Malignant Tumours	5	4	1
Innocent Tumours	32	32	0
	47	46	1

The fatal case was that of a man aged 68, who suffered from a large epithelioma of tongue and floor of mouth. It was successfully removed by Whitehead's method, but erysipelas supervened, and he sank on the eighth day after the operation.

There was one remarkable case of excision of upper jaw for

sarcoma. The median incision was used, and the tumour was scraped off the base of the skull. The operation was performed on 19th June, and patient was dismissed cured on 3rd July. He was seen again about three months afterwards, and there was no return of the tumour. He had been able to resume work, and complained only of some neuralgic pains on the side operated upon.

Lithotomy.—2 cases; 1 cured, 1 died. In the fatal case, the suprapubic operation was done, and two large stones were removed. The patient, however, was 75 years of age, very weak, and died exhausted six days after the operation.

Gastrostomy.—1 case, for carcinoma of œsophagus and cardiac end of stomach; survived eight days.

Hæmorrhoids, ...	5 cases,	cured.
Nævus, ...	2 "	"
Castration, ...	1 case,	"
Tenotomy, ...	7 cases,	"
Hare-Lip, ...	4 "	"
Re-section of Ribs, ...	1 case,	"
Stretching Sciatic Nerve, ...	1 "	"
Varicocele, ...	1 "	"
For Necrosis and Caries of Bone, ...	21 cases,	"
Miscellaneous Minor Operations, Abscesses, Removal of Foreign Bodies, ...	90 "	"
Total,	133	

Summary of cases seen at the out-patient clinics, prepared by Mr Hope, out-patient clerk.

Cases.	Cases.
Cuts and Bruises, ... 138	Localized Paralyses, ... 19
Abscesses, ... 124	Flat-Foot, ... 17
Ulcers, ... 85	Cystitis, ... 16
Gonorrhœa, ... 17	Urethral Stricture, ... 13
Syphilitic Disease, ... 101	Vesical Calculus, ... 1
Lymphadenoma, ... 2	Neuralgia, ... 5
Simple Tumours, ... 36	Congenital Malformations, ... 45
Malignant Tumours, ... 28	Hydrocele, ... 14
Inflamed Glands, ... 60	Orchitis, ... 11
Bronchocele, ... 4	Phimosis, ... 7
Cysts (Simple), ... 19	Paraphimosis, ... 3
Lupus, ... 10	Hernia, ... 22
Sprains and Bruises, ... 67	Foreign Bodies in Throat, etc., ... 17
Fractures of Bone, ... 61	Nævoid Tumours, ... 10
Disease of Bone and Periosteum, ... 87	Nasal Polypus, ... 6
Spinal Curvature, ... 53	Ozæna, ... 7
Joint Disease, ... 78	Hæmorrhoids, ... 7
Synovitis from Injury, ... 49	Prolapsus Ani, ... 3
Bursitis, ... 21	Fistula in Ano, ... 3
Burns and Scalds, ... 18	Fissure in Ano, ... 2
Ulcerated Throats, ... 20	Dog-Bite, ... 8
Tonsillitis, ... 10	Painful Cicatrix, ... 5
Skin Cases—Eczema, etc., ... 49	Warts and Corns, ... 9
Varicose Veins, ... 42	Other Cases unclassified, ... 25
Chronic Arthritis, ... 18	
Sciatica, ... 15	Total Number of Cases seen, ... 1487

Part Second.

REVIEWS.

A Manual of Dermatology. By A. R. ROBINSON, M.B., L.R.C.P. & S. E., Professor of Dermatology at the New York Polyclinic. New York: D. Appleton & Co: 1885.

EVEN were it not that Dr Robinson, as an alumnus and graduate of our university and a licentiate of our colleges, claims our recognition, his researches in the field of the morbid anatomy of the skin would, of themselves, render any book of his worthy of attention. The handsome volume before us is, he says, intended to be the basis of a future and larger, more pretentious, and more original work. One is almost inclined to wish that Dr Robinson had waited a few years till this project could have been more fully accomplished, and his reputation is such that it would have received no injury from the delay. As it is, the descriptions of the diseases are in many instances copied from the writings of others, while illness and numerous professional engagements compelled the author to fall back on Dr Gottheil, his assistant, to prepare a considerable number of the articles. It is, however, true that nowadays no one can compose a treatise on skin diseases which will not contain much material which has been already assimilated by passing through other minds.

Under the Anatomy of the Skin, which is comprehensively dealt with, there will be found a full account of the tactile and Pacinian corpuscles, illustrated from the author's own observations. The physiology is scarcely so amply treated of as one might have expected; and Unna's studies on the perspiration lead us to doubt the view held by Robinson, that the gland proper of the sweat apparatus furnishes all the watery liquid which reaches the free surface, and that none comes from the papillary blood-vessels or duct of the sweat gland. The classification adopted is that of Hebra; the one favoured by the American Dermatological Association is not thought well of, because its acceptability was decided by ballot.

Nothing specially deserving of notice will be met with on scanning the pages till we come to impetigo contagiosa. This is admitted as a separate disease, chiefly in deference to the opinion of others, for in nearly all the cases Robinson has seen, which correspond to the received characters, the condition was secondary to other diseases, and especially associated with the presence of pediculi. He explains its origin as being from those affected with pediculosis, and as the pus from ill-nourished persons is especially contagious and auto-inoculable, the same disease is reproduced in others. "Hence for the exclusion of pediculi as the cause of the eruption in

a given case, it is not sufficient to prove them absent in the case of the person under observation, but also in the individual first attacked." Syphilis is placed among the acute contagious inflammatory diseases on account of its similarity in many respects to the ordinary exanthematous affections. The characters peculiar to these eruptions met with in syphilis are well described, and the special course which the lesions pursue from their first appearance to their involution will be found to be well told. The pigmentary syphilide, to which of late considerable attention has been directed, finds a place; but there is no allusion to the opposite state, leucoderma syphiliticum. Dr Robinson subscribes to the opinion that mercury is the antidote to the syphilitic poison, and is indicated in all stages of the disease. The directions for its administration are ample and judicious. As to stimulants, the statement is made that wine and beer can be partaken of in moderate quantities, while brandy, whisky, and gin should be avoided. This rule, we think, too wide. In the young and in those otherwise healthy who are affected with syphilis, all forms of alcohol are, in our opinion, best avoided; in the weakly or broken down their use *may* be required. There is no notice taken of the value of chromic acid in the mucous patches which affect the mouth. Lichen planus is not only separated from lichen ruber in point of classification, but the wide difference which exists between these two diseases in Dr Robinson's opinion is emphasized by there being an interval of two hundred pages between their descriptions. Lichen planus he regards as an exudative disease, and its papules are the result of a circumscribed inflammation of the papillæ and upper part of the corium, while the changes in the epidermis are secondary. The hair follicles and sweat ducts are normal, and the former have no influence in determining the situation of the papules. Lichen ruber, on the contrary, he associates with psoriasis,—it is a keratosis, an hypertrophy of the epidermis, and not an inflammatory disease. The sweat glands are normal, except their ducts in the corneous layer, and the hair follicles are unaffected except at their orifices. Yet Robinson describes two varieties of lichen ruber, as Kaposi does, with a flat and with an acuminate papule; and it is not at all very easy from the account given of the lesions to discriminate clinically the flat form from his lichen planus. The views are ingenious, are supported by microscopical observation, but before being generally accepted they will certainly need further confirmation. No mention is made of the warty form of lichen planus; and while it is certainly true that arsenic sometimes unaccountably fails to cure lichen planus, it is, we think, in the present state of our knowledge, as to the disease or diseases in question, an error to say that it should not be given, as it frequently aggravates the eruption. If Dr Robinson's views are correct, taking the test of treatment, lichen planus is the rare, lichen ruber the moderately frequent disease, since the large majority of cases referable to lichen planus

as ordinarily accepted are curable by arsenic. Lichen scrofulosus is described, but there is no allusion to lichen circumscriptus or marginatus, or to pityriasis maculata, which is almost certainly identical with the latter, and which has been met with in America. Prurigo, which is looked on as a disease *per se*, is well described, but under the therapeutics there is no notice taken of pilocarpine, though this remedy has acted favourably in many cases. Hydroa is classed as a separate disease entirely, and as distinct from erythema multiforme, from pemphigus, and from herpes gestationis. The description given is worth careful study, and is the result of well-weighed estimation of what has been previously written on a disputed subject. One is, therefore, somewhat astonished to find in the January number of the *Journal of Cutaneous and Venereal Diseases* an article by Dr Robinson, in which he includes under hydroa, impetigo herpetiformis, dermatitis herpetiformis, pemphigus hystericus, and herpes gestationis, though in the systematic work we are now considering the first and last of these are treated separately, and without reference to hydroa. Pompholyx includes the cherio-pompholyx of Hutchinson and the dysidrosis of Fox, and here we agree with Robinson. He has investigated the anatomy of the vesicles in one well-marked case, and proves that there is no anatomical connexion between these and the sweat glands, thus setting aside the term dysidrosis. He looks on the disease as a neurosis, and not a catarrhal inflammation like eczema. It occurs in persons of a nervous temperament, or whose constitution is below par. Such sweat easily, and especially about the hands and feet, hence the confusion as to the exact nature of the disease. Sycosis is one of the skin affections to the elucidation of the anatomy of which Robinson early devoted himself. And this article is not only the best we know on the subject, but is thoroughly worthy of the author.

Eczema, too, which may be regarded as the touchstone whereby to try a work on dermatology, is satisfactorily handled. From the treatment we extract the following:—"Where large surfaces are affected with acute eczema, I have lately used with satisfactory results a preparation of oxide of zinc, mixed with mucilage of acacia and glycerine, as recommended by Unna of Hamburg. The advantages of the preparation are its cheapness, the ease of its application, and the completeness of the protection to the inflamed surfaces. It is compounded as follows:—

R. Zinci oxidi		℥j.
Mucilaginis acaciæ.		
Glycerine		āā ℥ij.

It can be applied with a brush two or three times a day. If there is much itching 1 per cent. of salicylic or carbolic acid may be added. In some few cases the glycerine has irritated the skin too much."

Psoriasis is another disease in which the views of Robinson have been almost universally and without reservation accepted. His description of the morbid changes in the skin have been confirmed, but have scarcely been modified. Speaking of elephantiasis, and its connexion with the filaria, the misleading statement is made that these minute embryonal worms are to be found in numbers only during certain hours of the day: the fact being that during the day they are rarely seen, but appear between the hours of six and seven in the evening, reaching their maximum at midnight. Clinically, lupus, in both its forms, is well described; but while the author agrees with those who consider that it is related to the ordinary scrofulodermata, and in speaking of these says that the essential cause of scrofula is, of course, the specific infecting agent, the bacillus, he is altogether silent as to the relation of the bacillus to lupus, or even as to its presence in the giant cells. While we have pointed out some of the defects, and alluded to a few discrepancies which we have met with during a perusal of this volume, it must not be thought that we are dissatisfied with it as a whole. On the contrary it will be found, in most cases, a reliable guide, and no little labour must have been expended on its preparation.

Von Ziemssen's Handbook of General Therapeutics. Vol. III. Respiratory Therapeutics, by Professor M. J. OESTEL, M.D., of Munich. Translated by J. BURNEY YEO, M.D., F.R.C.P., Professor of Clinical Therapeutics in King's College, London; Physician to King's College Hospital, etc., etc. London: Smith, Elder, & Co.: 1885.

THIS volume will, we anticipate, be one of the most important of the series, for the subject with which it deals has not hitherto been so fully treated of. While it may be regretted that the author has confined himself exclusively to what may be called the direct or local method of treatment, it need hardly be made a serious objection, as the knowledge of the ordinary methods of medication, by the administration of remedies through the stomach, is pretty general, and much of the literature is fairly accessible.

After a short historical *résumé*, the chemical part of respiratory therapeutics is taken up. This section occupies half the volume, and is devoted to the subject of inhalations. It is necessarily prefaced by an argument to prove that not only fluids and vapours but also solid bodies find their way into the remotest recesses of the pulmonary tissue. It has been contended, and is still held by many, that medication by inhalation in pulmonary affections is wholly vanity; but in the light of our pathological knowledge of coal-miner's lung and analogous states, and from the results of

physiological experiments, there can be no reasonable doubt that medicated vapours and fluids find their way into the respiratory passages.

The various apparatus used are described—both those for the pulverization of medicated fluids, and those for the development of medicated vapours, either by artificial volatilization or administered by means of respirator-inhalers. All the different substances applicable to any of these methods are passed in review, and are too numerous to be referred to more specially.

This section concludes with the treatment of special maladies by inhalation, and comprises the treatment of hæmorrhage from the respiratory passages, diseases of the nasal passages, mouth, and entire respiratory tract.

These methods have been much more elaborately tested in Germany than in this country, and the translation of this volume will probably give an impetus to their use here. We may not be so sanguine of great results as the author appears to be, but from the fairness and accuracy with which he has stated the result of treatment in affections in which we have used almost identical methods, we should be prepared for results beyond what we should have anticipated before we perused this work.

The second part of the volume is devoted to the physical part of respiratory therapeutics—that is, to the treatment by alterations of air pressure. Here also considerable space is devoted to the description of apparatus and to theoretical considerations and experimental evidence. We have witnessed the beneficial action of compressed air in some respiratory affections, and are rather surprised to find that in asthma it is not always safe to adopt this treatment; while the results recorded by the author as obtained by the pneumatic treatment of emphysema are somewhat startling.

We commend the volume to the favourable consideration of the profession, who will find much suggestive material in it presented in good English, thanks to the translator, who is himself well-known as an able labourer in a like direction.

A Handbook of Therapeutics. By SYDNEY RINGER, M.D., Professor of the Principles and Practice of Medicine in University College, Physician to University College Hospital. Eleventh Edition. London: H. K. Lewis: 1886.

WE have on previous occasions noticed favourably Professor Ringer's *Therapeutics*. The fact that this manual has reached an eleventh edition within a few years shows the great popularity of the work. It is well that such an excellent book should have an extensive circulation. The book is confined to Therapeutics, meaning thereby "the physiological and therapeutic action of individual drugs on the body." It contains no *Materia Medica* and

Pharmacy properly so called, and therefore can only be used as a text-book for Therapeutics. In these days, when students have so much to learn, we could have wished that the book had been smaller, but the way to accomplish this is to curtail the list of official medicines. The book is adapted to the new Pharmacopœia, and is sure to have, like the previous editions, an extensive circulation.

The Prescriber's Pharmacopœia, containing all the Medicines in the British Pharmacopœia. Arranged in Classes according to their Action, with their Composition and Doses. Revised and Edited by NESTOR TIRARD, M.D., M.R.C.P., Professor of Materia Medica and Therapeutics at King's College, etc., etc. Sixth Edition. London: J. & A. Churchill: 1886.

THE general plan of this work is excellent, and on the whole Professor Tirard has produced a Pharmacopœia which will be valuable to many prescribers. The great defect in the book is the fact that it includes "*all the medicines in the B. P.*" Many of these should never have been in any Pharmacopœia, and certainly are seldom if ever used by physicians who know the proper remedies to administer in the treatment of disease. If Professor Tirard would make a selection of those medicines which are really valuable, he would greatly increase the value and popularity of his work.

Another improvement of considerable importance would be the omission from their respective classes of those drugs, which, though possessing the peculiar physiological action of that class of drugs, are never used to produce that particular physiological action. Many drugs possess physiological actions, and yet these are of such a kind that they cannot be taken advantage of in the treatment of disease—for example, lobelia is undoubtedly emetic, but it cannot be safely used for the purpose of emesis; for a similar reason colchicum should not be classed under "*Purgatives*," for though it may possess this property, it is never used as a *purgative* in the treatment of disease. Notwithstanding these defects we have much pleasure in recommending the book to the notice of our readers.

The Pocket Pharmacopœia for 1885, including the Therapeutical Actions of the Drugs, etc. By ARMAND SEMPLE, B.A., M.B., M.R.C.P. Lond., etc. London: Baillière, Tindall, & Cox: 1886.

THIS volume, which is meant to be an epitome of the New Pharmacopœia, to which are added the therapeutical actions of the drugs, is one which we cannot recommend. The therapeutic actions are

so meagrely stated, that the book can never be used as a text-book by students. It is a book which can be of little use either to students or practitioners.

The Essentials of Materia Medica and Therapeutics. By ALFRED BARING GARROD, M.D., F.R.S., late Professor of *Materia Medica* and *Therapeutics* at King's College, etc., etc. Eleventh Edition. Revised and Edited under the supervision of the Author by NESTOR TIRARD, M.D., Professor of *Materia Medica* and *Therapeutics* at King's College, etc., etc. London: Longmans, Green, & Co.: 1885.

WE are glad to see that this popular and excellent text-book has reached the *eleventh* edition, embracing the alterations contained in the New Pharmacopœia. It is, however, not yet free from inaccuracies, as when the authors, at page 311, speak of the leaves of *Vaccinium Vitis-Idæa* having their "margins crenated." This is a very common mistake of writers on *Materia Medica*, but could be made by no one acquainted with the plant. Again, it is very misleading for students to be told that the cinchona bark, used to obtain the alkaloids, is "collected in the north-western and western regions of South America." True, South America is the home of the cinchonas, but equally true, the alkaloids are now obtained chiefly from cultivated plants. These and similar mistakes will doubtless be corrected in future editions.

A Practical Treatise on the Sputum, with special reference to the Diagnosis, Prognosis, and Therapeutics of Diseases of the Throat and Lungs. By G. HUNTER MACKENZIE, M.D. (Edin.), Lecturer on Practical Laryngology and Rhinology in the Extra-Academical School of Medicine, Edinburgh, etc. W. & A. K. Johnston, Edinburgh and London: 1886.

SINCE Koch's discovery of the tubercle bacillus, the sputum has received increased attention from physicians. The result is seen in such works as that now before us. It is not a large volume, but there is much in it that will help to throw a new light on respiratory disease. There are in all thirteen chapters. In the first three of these the characters of the sputum in non-tubercular affections of the lungs are considered. Attention is paid to the occurrence of Charcot's crystals and Curschmann's spirals, the latter of which appear to be better known in German than in English literature. It is also shown that elastic tissue may occur in some of these sputa, *e.g.*, that of chronic bronchitis. The presence of schizomycetes in certain forms of pneumonia

and pleuro-pneumonia is pointed out, but Dr Mackenzie is of opinion that they do not possess specific properties like the bacillus of tubercle. Two chapters are devoted to the consideration of the sputum in phthisis. The relationship of Koch's bacillus to non-tubercular diseases, to early phthisis, to advanced phthisis, and to the other constituents of the phthisical sputum, is discussed in Chapter V. Like other reliable observers, Dr Mackenzie has not found it in non-tubercular disease. In early phthisis he regards its presence in the sputum a most invaluable aid to diagnosis, and more important than the physical signs, affording as it does "the only satisfactory evidence of the existence of tubercular disease of the lungs."

The importance of a microscopic examination of the sputum in laryngeal disease is shown in Chapter VII. Portions of tissue spat up may enable a diagnosis to be made in cases that were previously obscure.

The therapeutic indications of the sputum in lung and laryngeal diseases are discussed in Chapters IX., X., and XI. In these, by means of the "bacillary scale" invented by the author, and described by him in a previous number of this Journal, he is enabled to show graphically the effects of climate and of antiseptic inhalations on the organisms. The results got from treatment by these or by drugs are not encouraging. Winter residence in high altitudes would appear to have more influence on the organisms than anything else, but this point has not yet been sufficiently studied.

A chapter on disinfection of the sputum, followed by another on the methods of examination, closes a work which, though not extensive, is sufficiently exhaustive and enriched with a large number of painstaking observations of great value. The illustrations, whether of organisms or tissues, call for much praise. The volume will repay a careful study.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXV.—MEETING VI.

Wednesday, 17th February 1886.—Professor GRAINGER STEWART, *President, in the Chair.*

I. ELECTION OF ORDINARY MEMBERS.

W. Greenfield, M.D., and Dr Oswald Wood, M.B., F.R.C.S. Ed., were elected Ordinary Members of the Society.

II. DISCUSSION ON ABDOMINAL SECTION, INTRODUCED BY MR LAWSON TAIT.

The President, in opening the meeting, said that before proceeding to the business of the evening—a discussion on “Abdominal Section”—he felt that every member and visitor would deem it appropriate that notice should be taken of the absence from amongst them of one who would have been one of the leading speakers on that subject if he had been spared to take his place there. He was sure every member had watched with the greatest interest the career of Dr Angus Macdonald, and with the greatest anxiety had heard the conflicting intelligence received as to his condition from time to time during the past few weeks. His death was a great loss to the profession in Edinburgh, and, in particular, to that department in regard to which they were looking forward to an interesting debate that evening. Every one sympathized with his relatives in the sad circumstances in which they were placed, and he was sure that as a Society they would desire to express their sympathy with them. They would now proceed to the business of the evening, which was to be a debate on “Abdominal Section.” They were fortunate in having among them one of the most successful operators in that department, and an old student of the Edinburgh Medical School, Mr Lawson Tait, and he had great pleasure in asking Mr Lawson Tait to open the discussion.

Mr Lawson Tait then said—The position which abdominal surgery has taken within the last fifteen years, and which it now occupies as an advance on its attitude at the beginning of that period, is wholly unexampled in the history of our art. I remember very well that my old master, Professor Miller, used to dismiss the whole thing in the very forcible words, “that abdominal surgery was abominable surgery;” and despite the strenuous advocacy of Sir James Simpson and Dr John Hughes Bennett, no kind of professional acceptance for it was obtained. I remember very well that I left Edinburgh in 1866, just twenty years ago, with many intentions, some of which have gone to form pavement, but with one which I felt more profoundly convinced would be maintained than any other, and that was, that whatever might be my fate in the ranks of my profession, I never would incur the awful responsibility and the odium which I had seen to accrue from this kind of work. In fact, I felt perfectly certain that if there was one operation I never should do, it was abdominal section. What a contrast this is to the actual fact! During these twenty years I have performed this condemned operation nearly 1400 times, and with a constantly diminishing mortality, even when the operations have been of the gravest kind. Thanks to the foresight and caution of Sir Spencer Wells, those of us who work in this department of surgery have fallen into the most

commendable habit of recording, in authenticated details, all our cases; and we publish them, at least those of us who do as ought to be done, in such a way and in perfect sequence of order that there can be no disputes—certainly there never has been the slightest dispute—as to the authenticity of the record. What disputes have arisen have been determined by difference in views about classification and other points which are not yet quite settled, but which are hardly pertinent to the broad issues which I wish to raise to-night. For my own part, I have followed the practice of publishing my results year by year, beginning on the 1st of January and ending on the 31st of December. Simply, therefore, as a foundation to the argument which I am about to lay before you, I want to draw your attention to two striking statements of fact.

First, That from the beginning of January 1884 to the end of December 1885, I have been responsible for the treatment of 139 cases of ovarian and parovarian cystoma; and I have accomplished the operations involved therein without a single death; and not only this, but there is not a single incomplete operation to charge myself with within that period where these two classes of disease were concerned. Let me also say that in these 139 cases the tumours varied in size from dermoid cysts of 6 or 7 ounces in weight up to huge multilocular cystomata weighing over 100 lbs.; that the complications involved in them were endless and varying in their severity; that adhesions were met with of every possible kind, and in connexion with every internal pelvic and abdominal organ; that the pedicles were of every shape and variety, from the long narrow connexion which is secured in a few seconds by the application of a ligature, to the sessile insertion where there was no pedicle at all, and where the whole mass had to be enucleated from its uppermost points down to its deepest pelvic relations; that in some such cases, three in particular, neither cautery, nor ligature, nor any other hæmostatic was of the slightest use, and I had to secure the bleeding points by catch forceps, leaving these closed up in the abdomen for many hours and then reopening it and removing them. There was, in fact, no kind of difficulty which was not embraced within these 139 cases, and yet without exception they recovered.

The second statement of fact is—That I have now performed 113 exploratory incisions, and that amongst these there can be reckoned only three deaths; but even these should not be credited in the least degree to the performance of the exploratory incision. What was the real fact is, that the exploratory incisions having failed to enable me to relieve my patients, they died of the progress of the disease, but within such time as that they could not be fairly said to have recovered from the operation. With such remarkable results as are here displayed within the short space of my own experience from the time when I left this hall a recently

admitted licentiate of this College, to the time that I now return to it honoured by a request to address this distinguished assemblage, hardly yet having approached middle life, I think I am justified in what I say, that no department of our art has shown such striking advances. It is perfectly true that almost within the same time ophthalmic surgery has jumped from a position of routine practice to that of almost an exact science; but it did not start, as my own department did, in the midst of hostility, not passive, but of the most active, and sometimes vindictive, kind.

A most interesting inquiry is at once opened by these two statements of fact as to how we can explain the startling contrasts achieved within the short space of twenty years. It is possible now to give perfectly satisfactory answers upon most of the main points of the inquiry, in spite of much throwing of dust, which has been practised upon those who were anxious to arrive at the facts of the case. Historical investigation shows that the first ovariectomy by Houston of Carlisle was successful, and he used a short incision, only four inches, and he tied the pedicle and dropped it back. McDowell was successful, and he followed Houston; so was and so did Nathan Smith. In England a wider departure from this practice was adopted: incisions from 8 to 18 inches were made, the long ligature and the clamp were used, and so from Clay down to Spencer Wells a mortality of 25 per cent. was maintained. Baker Brown brought the death-rate down to 12 per cent. by using the cautery and closing the abdomen. Keith rapidly improved on this. In my second series of fifty cases of ovariectomy, my mortality was 6 per cent. It fell to 4, then to 2, and you have heard that in my last series no mortality has occurred. My belief is that we have banished all deaths except those that must arise occasionally from tetanus, bronchitis due to the anæsthetic, and accidents which cannot be foreseen or avoided.

Mr Spencer Wells was very anxious to attribute the increasing success to the employment of the system known as Listerism, but that has been absolutely banished from my practice since 1881, and Sir Spencer Wells stands alone now in his argument.

There can be no doubt that to the intraperitoneal treatment of the pedicle and to the shortening of the incision a very large share of our success is due. But there are other factors too important to be overlooked, and I prefer to take them in the order of their importance, as it seems to me.

The factor of the subdivision of labour, or, as it is generally known, specialism, is, I believe, a leading feature in our success. There is a fashion to cry down specialism, particularly on the part of those who are injuriously affected by its progress, but this cry will do little to injure its prospects. It has been a royal road to success in every other walk of life, and the world is too far on in the path of social evolution for this weapon to be excluded from the fight with disease. The specialism of ophthalmic surgery has

proved an infinite boon to humanity, and no one would like to see the distinguished President of our own College of Surgeons supplanted by one who practised midwifery and attended measles and scarlet fever. Equally, the specialism of gynecology promises well, and it is, most curiously, already quite separated, south of the Tweed, from midwifery, with which it has been so long and inextricably mixed up. Most assuredly I believe it to be true, that any man desirous of being able to remove a hundred ovarian tumours with only two or three deaths, should separate himself from what is known as general practice, and devote himself specially to the work. That there are one or two brilliant exceptions is true, but it is only too certainly the rule that those who are mixed up with midwifery and with general surgery fail to get results which can justify their continuing to perform abdominal sections.

It is equally true—in fact, it is the mere appendix to what I have said—that men to whom it is likely that it will fall only once or twice a year to perform ovariectomy do it badly and unsuccessfully. It would ill become me to condemn such men, and I would not do it were it not that the condemnation comes from a far higher source than I can ever be.

James Syme was not only the greatest surgeon I have ever seen, but he was the greatest teacher of medical ethics I have ever listened to. I never can forget a lecture he gave us about a quarter of a century ago on methods of success in practice; and however imperfectly I may have carried his principles into practice, I know that what success I have had in life has been due to my efforts to follow them. “There are three ways you may try,” said the great master, “there are three interests you have to consider, and it will depend upon the order in which you consider them how success will be measured out to you. The first interest is your own, and it may seem to you the greatest, whilst it is really the least. The second interest is truly greater, for it is the interest of your professional brothers; but the last is the greatest of all, for it is the interest of your patient, and with that is eternally related the interest of the art you practise.”

More wholesome words than these have rarely been heard; and just as I am satisfied that no one now rushes into performing operations for cataract who is not, or at least has not some prospect of becoming, an ophthalmic surgeon, so I think the laparotomy epidemic ought to be a good deal restricted, not in its application, but in the number of those who venture into it. In Syme's own words, the real interest of the whole business is the interest of the patients upon whom we practise, and unless we secure for them the best possible results we injure them, we injure ourselves, and, worst of all, we injure the art to which we have devoted our energies.

In the early days of our practice, I speak for myself of the years

from 1867 to 1877, we were wont to credit much of our failure to causes outside the lines of our personal relations, and a great deal of nonsense was talked and believed. Keith altered much of this by boldly asserting that we ought to look into the details of our operations for causes of failure, and not blame things outside. In my own case, I believed that the air of the town had a prejudicial influence, and so we moved our hospital out into the country, and had complete isolation of each patient, with numerous and troublesome precautions for disinfecting the rooms after every operation. All these, or at least most of them, have been proved to be useless, for I have a large private hospital right in the middle of the town, and I have dropped all the precautions one after another with no ill effects. In fact, Dr Emmett of New York, who has been to inspect my proceedings, has gone so far as to say that "circumstances may favour me by living in a manufacturing district where the atmosphere is thoroughly charged with carbon, which in the form of soot penetrates everywhere." But in the same manufacturing district, in the Birmingham General Hospital, the ovariectomy mortality is 35 per cent., so that smoke is no safeguard. I use the old building which used to be the public hospital for my private patients, the only difference being that we used to have the out patients in the basement, and now there are none.

Following the mistaken views of Sir Spencer Wells, I used to have a great superstition about people bringing infection from outside, and I had an especial horror of zymotic diseases. I used to insist on everyone coming to my operations declaring that they had not been near any source of infection for seven days; now I never trouble to ask them where they have been. Two unwitting experiments settled this question for me. Some years ago a young girl came to me from Cornwall for an ovariectomy. Whilst removing the stitches on the seventh day I noticed that she was desquamating, and I found on careful inquiry that there could be little doubt but that she had scarlet fever at the time of the operation so slight as to attract no notice. I need not say that we took every precaution against the disease spreading, and I determined to do no more operations for a fortnight. But I was too late; I had carried the disease into my private hospital, and in the two institutions eleven cases of abdominal section, and three nurses, caught the disease without any of them being one penny the worse, save one of the nurses, who had a very sharp attack of articular rheumatism as a sequel.

On another occasion, I had been touching a child convalescing from scarlet fever, and desquamating, when I was called upon to re-open an abdomen on account of violent and sudden hæmorrhage. In the hurry I did it without even washing my hands, but the patient suffered nothing in consequence, though I must have had scales with scarlet fever germs in abundance on my hands. I need not say that I would not deliberately allow access of any specific

poison to one of my abdominal sections any more than I would allow it to any one else, but I am sure we have talked nonsense enough on this subject.

We also have talked very much against the performance of these operations in general hospitals without a clear recognition of the fact that conditions in these institutions may vary very much. We have blamed the institutions rather than the men who have operated in them. Keith's results in the Edinburgh Infirmary show that in skilled hands the propinquity of other cases is no bar to success. In a smaller way I have found the same thing. I have performed thirteen of the more serious operations of abdominal surgery in the general wards of general hospitals with uniform success.

I fancy, therefore, that we must begin to saddle upon the back of those who do these operations the major part of the blame for want of success.

The committee of my own hospital took this bold and unprecedented step in their last annual report to the subscribers. The mortality of the abdominal sections in our hospital last year rose to 9·8 per cent. as against 5·6 for the preceding year; and the committee promptly told my colleague Dr Savage and myself that they held us mainly responsible for want of success. We did not quite like the idea, but within limits there can be little doubt that the view is correct, and the responsibility of defending and defining the limits must lie with the operators. The apparent fact is, that we judge of results everywhere and in everything by success; and the feeling that we are to be judged by results, benefiting by those that are satisfactory and suffering from those that are bad, must form the best incentive for the exercise of that constant supervision over every detail which alone can secure success. If this is not so, why are those of us who are successful so ready to display our tables, and why do we hear so little from those whose lists would display very different accounts? There can be no doubt of the necessity for the introspection advocated by Dr Keith. The more it is carried out the better will be our work.

I am desirous at this time to discuss general principles rather than details, and therefore I come at once to the application of the successful issues already detailed to the extension of our particular department.

No sooner had Keith shown us the main lines of success than another great advance followed. The rule had been to temporize with ovarian tumours as long as possible, to tap them and tinker with them, to do anything rather than submit the woman to a risk of 25 per cent. But with 10 per cent. it was different. Bantock and I at once raised an outcry against tapping and delay, and we advocated in and out of season the practice that ovarian tumours should be removed as soon as they were recognised. Our views are now almost universally adopted, and in this way another

reduction in the mortality of 6 or 8 per cent. was brought about. Old adherent tumours which have been tapped are now the exception, whilst fifteen years ago they were the rule. Not only has the primary mortality been thus reduced, but I believe the secondary mortality has also been enormously diminished. I believe that the number of cases dying of cancer some months after ovariectomy is now very much less than it used to be. I cannot support this belief by figures, but I can support by figures the fact that unfinished ovariectomies are practically banished from my practice; and this is a conclusion pregnant with suggestiveness about the co-existence or future occurrence of malignant disease.

A 5 per cent. mortality in ovariectomy banished our fears of the peritoneum, unshackled our hands, and set us thinking what else we could turn them to. Most of my senior brethren became alarmed, their natural and very praiseworthy conservatism brought forth remonstrance and even unkindly interference. Even the great master in the North complained of the "restless surgery of the present day, which would leave nothing alone." But progress was a matter of necessity. The first brilliant achievement, one which greatly excited my envy, was that of Graham of Liverpool, to whom was brought a patient upon whom Spencer Wells had pronounced the doom that nothing could be done. Graham, as he has himself graphically told me, discussed with the patient the alternatives of inevitable death and "a chance of recovery." They decided upon an operation, and Graham made an exploratory incision, went on, and successfully completed an operation so bad that anything after it seemed possible. Here was a new departure—the great master of abdominal surgery vanquished by a novice, and the great principle of exploratory incision established.

Many cases came under our notice in which it was by no means clear that the disease was ovarian tumour, yet the unwritten law of Spencer Wells was that none others were to be interfered with. It is true that he did meddle with a few cases not ovarian, but that was by accident, such as an occasional removal of the spleen; but the advanced practice steadily gained ground, though not without much opposition and adverse criticism. The field was fairly opened, however, when I ventured on my first list of hysterectomies, with a mortality of 50 per cent. The region of the kidney was developed by the marvellous case of Campbell of Dundee, in 1873, who removed a huge cystic kidney by mistake; and I advocated the removal of diseased kidneys before a London Board of examiners as early as 1871, and got ploughed for my audacity.

In the same year I was asked to see a case of intractable pelvic suffering, of years' duration, which I found to be due to a small pelvic tumour, not larger than a Mandarin orange. I advised its removal, and found it to be an abscess of the ovary; and I cured my patient. Here was another, and, as it proved, an almost inexhaustible field for enterprise. But it was also a new departure,

involving for abdominal surgery a new principle. Spencer Wells' rule up to this time had been absolute—nothing was to be done save in face of impending death. Mere suffering was not to be relieved. But I remember that when I was engaged in the practice of general surgery, as I was pretty actively from 1863 to 1870, that I did not hesitate to perform very severe operations for conditions which caused mere inconvenience, such as cleft palate and the removal of lipomatous growths on the back, or wens on the head. I saw a patient die of erysipelas after one of the operations of Taliocotius, and no one uttered a word of condemnation. I could see no reason why there should be one surgical law for the nose and another for the ovary or Fallopian tube; and having found that I could alleviate, or absolutely cure, suffering of the most agonizing kind by removing ovaries and tubes affected by chronic inflammatory disease, I carried out my plan, and it has been followed throughout the civilized world. As experience grew, a constantly diminishing mortality justified the attempts, and now the operation ranks as one of the most successful in surgery.

Uterine tumours for a long time occupied a position which it seemed the army of surgery would never successfully attack; but in 1872 it occurred to Hegar of Freiburg and myself simultaneously, that if menstruation could be arrested by removal of the uterine appendages, the disease might be conquered. On the 27th July 1872 Hegar operated, but his case died. On 1st August 1872 I operated; my case lived; the disease was absolutely cured, and my patient is now in perfect health. Hundreds of cases have since been operated on, and the proceeding has now a universal recognition. The huge sarcoous masses, for which hysterectomy alone was the remedy, were removed by me first with a mortality of 50 per cent. This fell to 35 per cent., and is now at 15 per cent. Keith has been foremost in the success of this operation, and I have no doubt that we shall speedily secure for it results almost equal to those of ovariectomy.

Mixed up with all these proceedings, mixed up with an amount of confusion for which the only excuse is ignorance, is a proceeding of an altogether different kind, to which I have never lent a kindly favour, and to which I am now disposed to offer a strong objection, that which is known as Battey's operation. This is intended, in the words of the author, to bring on the menopause, and thereby indirectly benefit symptoms belonging to the vague and indefinite order known as reflex. I have tried a small number of experimental operations in clearly defined cases, and the want of success has been sufficient to stop me. "Normal ovariectomy" is an operation requiring no skill, little experience, and hardly any judgment, and therefore has been extensively, and, I fear, somewhat indiscriminately practised. I have protested again and again against it, yet many whose voices are no louder against it than

my own blame me for it, accuse me of doing it, and generally get confused over the whole subject. I desire once more to say, that save when the seat of such organic disease as will explain genuine suffering, the uterine appendages ought not to be removed; and that those who attribute all the pelvic aches and ails of women to the ovaries and tubes, and rush in to remove them, are dangerous people. I don't say they are dishonest, but I say they are misguided. This kind of laparotomy epidemic is no worse, however, and certainly not more harmful, than the tenotomy epidemic which spread all over the world when Dieffenbach first introduced his brilliant and serviceable operations. Every oblique eye was made more oblique on another axis, and many club-feet were hopelessly destroyed,—results to be deplored, but common enough in all instances of human progress. New things—especially new drugs—are always done to death, and I greatly fear that indiscretion with such a new drug as chloral has done more harm than all the surgical indiscretions collectively.

To turn now to the last and really the most promising phases of my picture. The surgeon has trespassed bit by bit on the precincts of the physician, and I am bound to say a most loyal and encouraging reception he has had.

So long ago as 1745 Jean Louis Petit advised the surgical treatment of gallstones, but it rested till 1878, when Marion Sims carried out the proposal of Petit almost as that great surgeon described it. Sims' case failed, but my first succeeded in 1879; and since then I have done twenty-one in all, with uniform success. Here the argument for exploratory incision comes with perfect clearness and great force. Gallstones is a frequent, but not a very fatal disease. It is, however, accompanied by great inconvenience, and often intolerable agony. I have long since ceased to regard myself as a mere saver of life, for I think the destruction of pain and suffering is a much higher function. I have found that the mere exploration of the abdomen is a harmless proceeding; and as the diagnosis of gallstones can be made with accuracy only in about 75 per cent. of the cases, we are entitled to construct a rule upon their premises.

Given a case when the symptoms and signs are such as to make the diagnosis of gallstones probable, given the sufferings of the patient to be such as to make him feel equal to facing a 3 per cent. risk (and this is a question for him alone), I say open the abdomen, verify your diagnosis if that can be done; if not, correct it. If the diagnosis is correct, you can cure your patient; if it is incorrect, there is small risk of harm being done. The exploratory incision is the last implement of pelvic and abdominal diagnosis, but it is at the same time the first implement for permanently successful treatment. As a contrast, let me say that in so common an operation as the removal of the breast the conditions are often reversed. The operation treatment is completed, then the

exploratory incision is made, only to show the diagnosis utterly wrong.

I have here again been misunderstood and misrepresented, and I want distinctly to insist that I do not wish to make abdominal section take the place of other means of diagnosis or to supplant any, only to supplement all when all these have failed. The percentage of exploratory incisions, like the percentage of incomplete operations, will therefore form elements of great value in judging of the work of any one surgeon, or of the work of this department generally. Both ought to diminish with experience. Amongst my first 1000 cases of abdominal section the exploratory incisions rapidly diminished in successive hundreds. Of the 1000 abdominal sections there were 99 explorations, or nearly 10 per cent. Of 400 abdominal sections which I have done since then, there are only 13 explorations, or only 3.25 per cent., and incomplete operations hardly occur at all.

To what I have already said I need not do more than add a list of extensions of these principles to abscess of liver and hydatid tumours, abscess of the spleen, tumours of the kidney of various kinds, and stones in its pelvis, etc.,—a list of over a hundred cases of very painful and generally fatal diseases brought to a condition of cure with only a loss of two lives.

What I want to conclude with now is a brief speculation as to what we may do in the future, especially with chronic and even acute inflammatory diseases in the abdomen, and with the diseases there of the malignant type. Concerning the latter I again go back to Mr Syme—"What I cannot cure is malignant," and beyond this, in spite of our constant changes of nomenclature, we have never yet got. By dint of constant disappointment we have long since been able to define with considerable accuracy what cases we cannot cure, and amongst them are included certain characteristic diseases of the stomach, pylorus, intestines, and uterus. Resections and removals for these diseases have become fashionable in certain surgical schools, but I will have none of them. I have done none of them, deterred by Syme's principles—they are not in the interests of my patients. I daresay I might get glory by publishing half-a-dozen cases of resection of the pylorus, and within twelve months I could undertake to provide such a list, for numbers of these cases are sent to me. But in twenty-four months they would all be in their graves, and the art of surgery would have suffered by reason of six useless, unprofitable, unnecessary, and improper operations. Things which cannot be brought under Syme's rules cannot be good for anybody.

Far different from this is the condition of inflammatory disease, whether it be acute or chronic, and if my collective experience has established one conclusion for me more valuable than another, it is that the existence of peritonitis—acute, subacute, or chronic—is the most stringent of all reasons for the performance of abdominal sec-

tion. It would be a matter of very great difficulty for me to state accurately the number of times I have opened the abdomen in the presence of inflammatory disease, for of course in a very large number of my operations it existed as a condition more or less of subsidiary importance. Thus, in ten of the series of 137 ovariectomies I have alluded to, acute peritonitis was perhaps a greater reason for removing the tumours than the presence of the tumours themselves, for the patients were in the process of dying from the inflammatory disease and not from the tumours, the peritonitis arising from the rotation and subsequent strangulation of the cystic growths. So a very large number of my cases of inflammatory disease of the appendages have had peritonitis, acute or chronic, as their leading feature, and of course it was their great source of danger. It has come to be an axiom with me—accepted now, I think, by every one who practises in this department of abdominal surgery—that the presence of peritonitis, together with some other condition which would by itself involve abdominal section, is the most potent of all reasons for the immediate performance of abdominal section. But if we take cases where there is no other condition present except the peritonitis, which would demand interference, the peritonitis alone is an imperative reason for interference. In cases of the chronic disease associated with suppuration, or with the formation of those gaseous masses which have given rise to the name of tubercular peritonitis, the treatment of opening the abdomen, completely clearing out all effusion which can be reached, and subsequently draining the cavity, has met with a perfectly uniform success in my hands in its immediate results, and with a secondary success, that is to say, complete cure of the disease and the establishment of perfect health in at least 80 per cent. of the cases. Let me, as one exception to the scheme of this paper, briefly narrate the story of one case.

E. T., aged 18, was sent to me by Dr M'Carthy of St George's, Wellington, in April 1872. She was thin and emaciated to the last degree, and was hectic, with a night temperature of 38° , and pulse of 126; morning temperature falling to $36^{\circ}8$, and pulse to 99. The abdomen was distended and tympanitic. She had been seen by Dr M'Carthy on the 2nd of April, having been before that for some time under the care of an unqualified practitioner who had been treating her for bile, as it was said. The following are Dr M'Carthy's notes of the case supplied to me at the time:—

"The condition in which I found her was one of the greatest emaciation. Seldom have I seen a greater, save in the last stage of phthisis. The skin drawn tightly, as it were, over the cheek bones and hollows above the zygomas, the limbs so wasted as to make the bones almost demonstrable for a first year's student. She had been learning the dressmaking, and for that purpose had been in the habit of going to Wellington, four miles from this, every morn-

ing, and returning each evening. She had complained of pain in the side of the abdomen, which became more constant, and of great severity. Menstruation had first appeared at the age of 13, and had been regular until a month before I saw her, when she got a severe wetting during her walks about the time the period should have appeared. In addition to the general appearance of emaciation, the abdomen was larger, and the enlargement had been of rapid growth, and this I was induced to regard as being due to some form of malignant disease. The chest sounds and the general visceral indications were healthy; the night temperature was $101^{\circ}8$; pulse, 120. The chief symptoms were incessant vomiting and diarrhoea."

On my second examination, on the 16th of April, I became perfectly satisfied that the girl was suffering from chronic lymphatic peritonitis, and on the morning of the 17th I opened the abdomen and found it was so. I removed two pints of purulent fluid, and found a large number of masses of flakey, curdy lymph, of which I cleared out as much as I could, and left in a large-sized glass drainage-tube. The drainage-tube was removed on the 24th of April. The wound did not heal, but slowly opened through its whole length, and on the 12th of May large pieces of curdy-like slough, which doubtless were fragments of the hardened lymph which I spoke of, came from the wound, and these pieces went on coming, at a few days' interval, till the middle of June, some of them being nearly as big as half a hand. The wound then began to heal, and the patient was discharged perfectly well towards the end of July, having gained very much in flesh and strength. On the 31st of the following October I happened to be at Wellington Station, changing trains on my road home from a consultation, when I was accosted by a great, strong-looking, red-faced girl, who asked me abruptly if I knew her? I said I had not the slightest recollection of her face, nor did I know in the least who she was. "Oh," she said, "I am E. T., whose life you saved in April last." She has since remained perfectly well.

The trouble in such cases is this—how to determine the point at which surgical interference becomes necessary; and this, really, is one of the vexed and urgent questions of the moment upon which the physicians present may very profitably favour us with their views. The same point arises again in the cases of obstruction of the bowels—When should the treatment of the physician be subsidized by the appearance of the operating surgeon? The physician naturally cries out against the performance of operations too early. The surgeon deplors the performance of operations too late, and the just middle of the matter can be settled only by the careful discussion of these difficult questions in such assemblies as this. My own idea is that the rule should be that as soon as effusion can be distinctly determined, and as soon as a hectic temperature indicates that that effusion is purulent, abdominal section ought to be

performed. One of the most distressing cases with which it has ever been my business to be associated was one in which a most valuable life was lost by what seemed to me to have been most improper delay. The question of abdominal section was discussed in the middle of December, and it was arranged that my friend Dr Keith should perform the operation, on account of the presence of purulent effusion in the abdomen. By the interference of well-intentioned but most mistaken friends, this operation was not performed when it ought to have been. The patient was removed into my own neighbourhood, and within a few days, at the request of one of our most distinguished Midland physicians, I performed the operation, only with the result of finding that I could relieve but one or two of a possibly large number of loculi of purulent effusion. From the history I do not think there can be any doubt, that if Dr Keith had been permitted to perform the operation when it was originally suggested a brilliant success would have been the result. From other cases of a similar kind I have learned the lesson that there is a Scylla and a Charybdis; and whilst I do not desire to steer my bark against the Scylla of the physician, I must earnestly deplore the frequency with which I am wrecked on the surgical Charybdis.

For acute peritonitis I do not think the prospects of abdominal section are anything like so promising; but, out of four operations which I have performed for this disease, I am certain that I have saved two lives, and I do not think I could be contented to allow any case of puerperal peritonitis for which I was responsible to get into a moribund condition before I had cleaned out and drained the abdominal cavity. In such conditions, whether acute or chronic, the abdomen becomes an abscess; and here again I desire to protest against the application of one canon of surgery to the abdomen and another to the cavity of the knee-joint.

If our friends Professors Annandale and Chiene, or Mr Joseph Bell, or any other of the numerous surgeons now present, are called in to a case of suppuration, either acute or chronic, of any large joint, let me ask them if they would not immediately relieve its contents by an incision? The mere extent of the serous surface of the abdomen is no argument against the application of the surgical principle. And now that we have seen our way, by success in less disputable regions, to early interference, with an amount of evidence as complete as can be provided, that the interference itself does but little harm, all I plead for is, that we should be at least permitted by our colleagues who have medical charge of these cases to see what relief in them may or may not be achieved by interfering before a hopeless condition has been arrived at.

(To be continued.)

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING V.

Thursday, March 11, 1886.—Dr MACLAREN in the Chair.

Dr Lediard showed A PATIENT recovering from a bullet wound of the liver. The entrance wound was situated 4 inches below the right nipple and slightly to the outer side, between the seventh and eighth ribs. The wound of exit was over the last rib, 3 inches from the spine. The wound discharged bile-stained pus for some time. The skin and urine were jaundiced. There had been some local peritonitis and pneumonia. There had been no severe hæmorrhage.

Dr Lediard showed A BOY with a gland extruding from a wound in the neck. The boy had suffered for five years from enlarged glands in the neck, which had suppurated, and one of the glands had extruded itself through the wound thus formed.

Dr Barnes showed A PATIENT with psoriasis, affecting not only the trunk and extremities, but also the scalp and face.

Dr MacLaren showed A BOY whose elbow had been excised three months ago for ankylosis. The wound was left open. Passive movement was begun in eight days. Voluntary movement was now very fair, but had been slow of coming.

Dr Dowie read A NOTE OF A CASE IN WHICH PYURIA WAS ASSOCIATED WITH THE PRESENCE OF AN INTESTINAL WORM (*Ascaris lumbricoides*).

I have thought the following case worthy of record, in which bladder irritability and the presence of a large quantity of pus in the urine were associated with, and, in my belief, caused by the presence of an *ascaris lumbricoides* in the intestine. The urinary symptoms appeared coincidently with the other symptoms, which pointed to the presence of a worm. After the expulsion of the worm the urinary symptoms disappeared, and the pus gradually diminished in quantity, the urine becoming clear in seven days.

Annie M., age 3, was first seen by me on 1st February. She had been suffering for seven or eight days previously with symptoms which suggested the possibility of the presence of a worm. Shortly there were abdominal pain, capricious appetite, and restlessness at night, and, in addition, it was stated that her urine was thick and was passed frequently. Her temperature was 101°. On examination, the urine was found to contain pus in large quantities, and to be of acid reaction. I prescribed *santonine*, gr. ii., to be taken at bedtime, with a dose of castor-oil in the morning. This was repeated on the following night.

3rd February.—No worm had been passed. I now treated the cystitis, prescribing tincture of *hyoscyamus* and bicarbonate of potash.

5th February.—A round worm, about 4 inches in length, was passed this morning.

6th February.—The child slept well during the night. The pain in the belly was no longer complained of. The urine was passed at natural intervals. Pus was still present in the urine, but in smaller quantity.

9th February.—The child is reported to be in her usual state of health. The urine still contains a slight deposit of pus, and is of acid reaction.

13th February.—The urine is free from pus.

10th March.—I obtained a specimen of the urine, and on examination found no pus. The urine was loaded with urates. There has been no urinary trouble since the last report. I may add that there have been no thread worms, and no irritation of the external genitals. There seems to me little doubt that the pus was due to a reflex irritation of the bladder from the presence of the *ascaris lumbricoides* in the intestine. The complete disappearance of all urinary symptoms after the expulsion of the worm, and the gradual clearing of the urine from pus, seem to point in this direction. If this were the cause, it must be a somewhat rare one, as mentioned in none of the ordinary medical or surgical text-books, nor by Sir Henry Thompson in his lectures on diseases of the bladder, nor by Cobbold in his lectures on helminthology. Cobbold relates one case of a lady who had passed a round worm, and in whom for six months there still continued abdominal pain and bladder irritability. He does not say whether there was pus in the urine or not, but he looked upon the bladder trouble as a separate ailment, unconnected with the presence of the worm.

Dr Macdougall read a NOTE ON THE USE OF URETHAN. He had used it in several cases with good results. In one case of advanced aortic and mitral disease with dropsy and great restlessness, three to five grains gave good results. In a lady, with a mild attack of delirium tremens, urethan in fifteen grain doses produced sleep. He had used it in another case, in which a gentleman had maniacal attacks following the continuous use of bromidia for two months in doses of a drachm every night. The bromidia was stopped and fifteen grains of urethan substituted, with the result that the gentleman was perfectly well the next morning.

Dr Lediard read a NOTE ON THE USE OF PARALDEHYDE.

The following pathological specimens were shown:—

By Dr Barnes—FIBRINOUS CONCRETION IN THE RIGHT VENTRICLE, and a DISEASED KIDNEY.

By Dr Maclaren—LOWER JAW REMOVED BY OPERATION, and a UTERINE FIBROID.

By Dr Lediard—ADENOMA OF BREAST, SCIRRHUS OF BREAST, and MAMMARY ABSCESS.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

URETHAN.—Since October I have been using urethan in a variety of cases with satisfactory results. I have used it in over 50 cases as a sedative and hypnotic, and my experience of its action encourages me to recommend the drug to the readers of the *British Medical Journal*, believing that in certain cases it will prove of great value. The cases in which I have prescribed it were of the usual run of everyday practice, where a sedative and hypnotic was required; general restlessness, sleeplessness, neuralgia, catarrh, certain forms of skin affections with great irritation, also rheumatism and gout. Many of my patients had some peculiarity of constitution which prevented the use of opiates of the usual type; and it is in this species that I think urethan will prove of great value. One gentleman who had suffered from insomnia for weeks, and cannot tolerate opium or chloral, took 15 grs. at bedtime with the most perfect result. He wrote to me and said, "The sleep caused was most pleasant and refreshing. I awoke without a headache, with appetite for breakfast, and what was equally agreeable, there was no interruption to any of my functions." Similar testimony has been given by the majority of patients who have taken full doses to produce sleep. In smaller doses its action is less marked, still it is decidedly calmative and agreeable, causing no unpleasant effect, such as nausea, flatulence, constipation, or headache. It does not affect the nerve centres of circulation or respiration, but spends itself on the cerebrum. It possesses, therefore, great advantage over the older and valuable sedatives, which have certain evil influences, especially in exceptional cases. Given in gout and rheumatism in full doses, alone or in combination, it has the great advantage over morphia of not interfering with the action of the bowels or kidneys, besides it is not unpleasant to the taste; the only objection to it is its price, although that has been reduced 50 per cent. since I gave my first dose three months ago. (A. S. Myrtle, M.D., Harrogate).—*The British Medical Journal*, 20th February 1886.

URETHAN AS AN HYPNOTIC.—Dr Dickson draws the following conclusion from his investigations as to the action of urethan (*Deutsche Med. Zeitung*, January 25, 1886):—Urethan is an excellent cerebral hypnotic, and possesses advantages over the usual hypnotics in that it is well borne, produces absolutely no unfavourable symptoms, and causes sleep which

appears to be perfectly similar to the physiological state. Urethan appears to be indicated in cases in which the other hypnotics, either on account of their unfavourable action on the heart or respiration, on account of their taste, or for some idiosyncrasy of the patient, have to be reduced or stopped. The minimum dose is 15 grains, but in persons from fifteen years of age and upwards this may be increased to 60 grains without danger. The best mode of administration is in capsules or in water solutions, with some syrup to correct the taste.—*Therapeutic Gazette*, 15th March 1886.

ADONIDINE IN CARDIAC DISEASES.—It must always be difficult to find a remedy which can replace digitalis, and yet, in view of the inconvenience which the constant use of digitalis entails, a drug which would combine the advantages of digitalis and would be free from its disadvantages is greatly to be desired. Dr E. Durand (*Journ. de Méd.*, Dec. 1885) has made a number of comparative studies of various cardiac drugs in the hospital service of Dr Deplats, at Lille. The drugs which he has studied are digitalis, caffeine, convallarine, and adonidine. Digitalis, as is well known, is often hardly tolerated, does not act immediately, and cannot be given for any length of time without danger of cumulative effects, and is not admissible in cases in which the kidneys are diseased. Caffeine, whose advantages are not yet sufficiently known to practitioners, frequently produces insomnia and a state of nervous excitement which necessitates its withdrawal. Convallarine appears to be extremely irregular in its reaction, judging by the contradictory opinions which have been published upon it. Adonidine, the active principle of *Adonis vernalis*, appears to be free from bad effects, and all physicians who have heretofore employed it attribute to it properties analogous to digitalis. It increases the energy of the cardiac contractions, regulates the pulse, and diminishes its frequency. It is rapidly eliminated, and never occasions secondary action from cumulation in the organism. Finally, it increases in a marked degree the quantity of urine, and hence serves to remove dropsies and œdema. All these facts have been observed by M. Durand, with the exception that the influence on diuresis was only marked in one case, in which it doubled the quantity of urine passed in twenty-four hours. He concludes that the indications for its employment are the same as for digitalis. The daily dose employed was $\frac{1}{10}$ of a grain in pill form. In some cases the dose was increased to $\frac{1}{5}$ without producing any phenomena of intolerance.—*Therapeutic Gazette*, 15th March 1886.

GALIUM APARINE IN PSORIASIS.—The wife of a staff-sergeant in this garrison had psoriasis in the left hand for upwards of twelve months. She derived no benefit from chrysophanic acid ointment, tar ointment, or arsenic, and was unable to do any housework on account of the painful fissures and general soreness of the affected part. A friend advised a trial of galium aparine to be applied

locally as a poultice, and also to be drunk as an infusion. In less than three weeks the psoriasis disappeared, and the skin resumed its normal state.—F. W. Orwin, Surgeon-major, Exeter.—*The British Medical Journal*, 27th March 1886.

THE DIGESTION OF MILK.—Dr M. Reichmann draws the following conclusions from a number of elaborate experiments as to the digestibility of milk in the human stomach (*Deutsche Med. Zeitung*, No. 82, 1885):—1. Boiled milk leaves the healthy stomach more rapidly than an equal quantity of unboiled milk. 2. The digestion of boiled milk is more rapidly accomplished than that of unboiled milk. 3. The coagulation of unboiled milk in the stomach is complete in five minutes. 4. The coagulation is not caused by the acid of the gastric juice, but by the influence of a special ferment (milk-curdling ferment). 5. The acidity of the gastric juice is at first due almost solely to lactic acid, and late in the process of digestion to the presence of hydrochloric acid. 6. Hydrochloric acid appears first in perceptible amount forty-five minutes after the ingestion of half a pint of milk. 7. For the first hour and a quarter after the ingestion of milk the acidity gradually increases, and then decreases until the milk has entirely left the stomach. 8. The curds of casein in digestion of boiled milk are much softer than in the digestion of uncooked milk.—*Therapeutic Gazette*, 15th March 1886.

A NEW ANTISEPTIC.—Chiandi Bey has published a number of communications on the bisulphide of carbon, which, contrary to the statements of chemists, he states to be soluble in water, in the proportion of 1 part to 2000 of water. This weak solution, according to his observation, entirely prevents any fermentation, and therefore suggested itself to him as a good antiseptic. The author, in the *Centralbl. für Chirurgie*, 12th Dec. 1885, highly recommends this solution in nearly all cases where a disinfectant is required. He has employed it in the treatment of diseases of the mouth, and in suppuration with chronic inflammation of the mucous membrane. We have already referred to its use in typhoid fever in arresting the diarrhoea and disinfecting the stools.—*Therapeutic Gazette*, 15th March 1886.

THE CURE OF FISTULÆ BY INJECTIONS OF OIL OF TURPENTINE.—Cases of anal fistula in connexion with carious bone, fistula in connexion with the teeth, fistula of the duct of Steno, and atonic fistulæ of various varieties, have all been treated with good results by Dr S. Cacchini by the injection of oil of turpentine (*Wiener Med. Blätter*, 14th January 1886). The reason for the employment of this drug is attributed by the author to the fact that oil of turpentine, on the one side, is a powerful stimulant to the formation of granulations; and, on the other hand, is an antiseptic; while, when employed with ordinary care, it can produce no un-

favourable results. His results are based on a large number of cases, which are reported with very great care and accuracy, and in every appearance seem to warrant reliance on the author's statement. A permanent cure was stated to have been procured in a large number of cases. First, as regards anal fistula, he details seven cases of fistula in four individuals, in whom the injection was repeated several times at intervals of three days. The author recommends the employment of syringes with blunt nozzels, then the closing of the opening of the fistula with the finger after the injection has been made, so as to insure thorough contact. The pain produced is slight, and is quite bearable, although, if necessary, the oil of turpentine may be diluted with olive oil. In five cases the fistulæ were completely cured. In one the patient stopped the treatment before a perfect cure resulted, as he was satisfied with the cure that followed. In another case of a complete sinuous fistula in a decrepid man, considerable improvement was produced, and after finally being operated upon the fistula was cured. In cases of fistula in connexion with carious bone, four cases are reported in which cure was completely produced in from two to three months. It is hardly necessary to analyze his other cases, except to add that in all a favourable result appears to have been obtained.—*The Therapeutic Gazette*, 15th March 1886.

PERISCOPE OF MEDICINE, CLINICAL AND PATHOLOGICAL.

By G. A. GIBSON, M.D., and WILLIAM RUSSELL, M.D.

THE PATHOGENY OF INFECTIOUS DISEASES: MICROBES, PTOMAINES, AND LEUCOMAINES.—Dr F. de Ranse discusses this subject in a communication to the Academy of Medicine. With the number, every day increasing, of the microbes found in pathological products, and to which are attributed a pathogenic action, the class of infectious diseases tends also to grow and to approach more and more to that of the parasitic maladies with which some already identify them. The general idea of a pathogenic microbe is that of a living organism possessing an individuality, descended from a long line of ancestors like itself, and transmitting its properties to its descendants; offering in its fully developed or its spore form great resistance to destruction; preserving in indifferent media its aptitude to develop; contained and carried about in the media which surround us—atmosphere, water, soil, etc.; waiting its opportunity when the vital economy, in consequence of predisposition, of injury, or of some circumstance or other, offers a favourable medium for its development. Then the microbe fixes itself, nourishes itself, develops, reproduces, multiplies, at the expense of the individual who has received it, and by various acts produces in him a disease, which is always the same for the same microbe,—so much so, that the specificity of a malady has for its correlative term the

species of microbe to the action of which it may be attributed. This manner of regarding the pathogenic action of microbes in infectious diseases accepts as demonstrable two things:—1st, Fixity in the species of pathogenic microbes; 2nd, A uniform mode of action on their part, so that they produce a given disease. Microbes are known by their form, or their reaction to staining fluids, or the chemical products they engender, and by none of these tests does it appear that there is fixity of species; the same microbe may at different periods of its existence be a coccus, a rod, a filament, or a spiral; the reaction to staining fluids depends on the medium in which the microbe is nourished; while, most important of all, they may be so cultivated that they lose their pathogenic property. These microbes may develop and multiply at the expense of the tissues or humours of their host like true parasites, or, on the other hand, the substances which they engender may act as poisons or intoxicants. Thus in the infectious maladies there are two ways in which the part played by pathogenic microbes may be regarded,—the one resembling parasitic affections, the other intoxications. He seems to accept Gautier's work, showing that infectious maladies may originate and develop without the intervention of pathogenic microbes, by a simple deviation in the nutritive processes, by an incomplete elimination of the leucomaines which are incessantly formed by the vital processes of the cells forming the tissues and organs of the body. To these leucomaines are doubtless added ptomaines engendered by the microbes which we shelter, and which aid in the accomplishment of certain physiological processes. From the above point of view we can see how an infectious malady once engendered is able to be transmitted, by furnishing certain microbes with a medium sufficient to modify their characters and to give them pathogenic properties which they did not possess formerly.—*Gazette Médicale de Paris*, 6th March 1866.

ACETONEMIC DIABETES.—Cornillon and Mallet in 1883 showed the semeiological import of diabetic coma, and deduced the following conclusions:—1st, In the immense majority of cases of diabetic coma, acetonemia cannot be invoked as the cause of this accident; 2nd, The reddish-brown coloration with the perchloride of iron, and the rose colour with sulphuric acid, is not only observed in the urine of those succumbing to diabetic coma, but also in emaciated, enfeebled persons who have long had glycosuria; 3rd, These reactions are not pathognomonic of the presence of acetone; they are not in direct relation to the quantity of sugar found in the urine; 4th, The reactions are not observed in plump diabetics even after the affection has continued long. The following year they showed that in the majority of cases of diabetic coma, as well as in certain emaciated and worn-out glycosurics, it was not to the presence of acetone that we ought to attribute the cerebral phenomena in the one or the disorders of nutrition in the other, but

rather to the existence of an alkaline sulpho-cyanide in the economy. They believe there is only one form of acetonemic diabetes, although there are many modes of termination of the morbid process—the one rapid, the other slow. The acetonemic diabetic is incapable of all physical and intellectual labour. The memory is defective. Ideas are a little confused. The sentiments are blunted. He is weary and somnolent. The appetite is good, and the digestive functions are normal. In this class of case four principal symptoms arrest attention: 1st, rapid and considerable emaciation; 2nd, extreme feebleness; 3rd, urinary troubles; 4th, the special odour. The quantity of urine is very large and very pale, and the quantity of sugar is very great. The quantity of the sulpho-cyanide present is usually, but not invariably, in direct relation to the quantity of sugar. The sulpho-cyanide is never found in diabetes insipidus. The special odour is rarely noted, but when present it resembles ether or chloroform. Death occurs in two different modes—rapidly by coma, or a veritable attack of delirium tremens. In this case the urine gives the reactions specified, and contains a notable quantity of sulpho-cyanide of potassium. The patient may have been over fatigued—the fatigue not necessarily having been great—by a railway journey, and accompanied by privations of food or fluids, and this may have precipitated the condition. Those, on the other hand, that die slowly often do so of pulmonary phthisis.—*Le Progrès Médical*, 10th April 1886.

ELEMENTS OF PROGNOSIS IN BRIGHT'S DISEASE.—The late Dr Austin Flint, of New York, in a paper read before the New York County Medical Association, sums up the elements of prognosis in Bright's disease as follows:—1. Acute Bright, as a rule, does not tend to a fatal result, nor to eventuate in a chronic affection. 2. A sub-acute diffuse nephritis, having the same seat and character as acute Bright, exclusive of acuteness, occurs not only after scarlet fever and other infectious diseases, but irrespective of these. Occurring in other pathological connexions, or as a primary affection, it is liable to be overlooked, or may be mistaken for a chronic form of Bright. 3. Acute Bright, or a sub-acute nephritis, not infrequently occurs as an intercurrent affection in the course of Bright's disease. 4. A susceptibility to the causes of inflammation of the uriniferous tubules, irrespective of the existence of chronic Bright, is to be recognised as an individual peculiarity. 5. Chronic Bright may exist as a latent affection for an indefinite period, provided the danger to the kidneys is no more than equivalent to the loss of one of these organs, and that the functions of the other organs of the body are well performed. The disease may be tolerated so long as the renal affection does not progress, and other conditions are favourable. 6. The adequacy of the functions of the kidneys has relation to the amount of excrementitious elimi-

nation by these organs, and this is determined by ascertaining the specific gravity of the urine and the quantity passed in a given time. 7. The danger from the accumulation of urinary constituents in the blood cannot always be determined by ascertaining the amount of renal inadequacy, owing to variations in the efficiency of vicarious elimination and in the tolerance from habit of these constituents. 8. Uræmic coma, occurring in cases of chronic Bright, may be incident to an intercurrent temporary nephritic inflammation or to accessory conditions, so that, if danger can be averted, restoration to the state of health which existed prior to its occurrence may be hoped for. This statement also applies to acute pulmonary œdema. 9. Renal asthma is generally, if not always, a fatal prognostic.—*Journal of the American Association*, 9th January 1886.

IDIOPATHIC HEADACHE.—By the term idiopathic headache is meant a headache *per se*, not symptomatic of any definite lesion. Migraine is its most ordinary form. It may be due to either anæmia or hyperæmia of the brain, the result of some vaso-motor disturbance. The two varieties are termed angio-paralytic and angio-spastic. The pain is due to direct pressure on the nerves that traverse the walls of the bloodvessels, or those ramifying on the meninges. The retina is congested and scarlet red in the congestive form. In the anæmic it is pale. Treatment should aim at restoring the equilibrium of the intra-cranial circulation, to overcome vascular spasm in the one case, and to counteract relaxation in the other. Certain remedies have a special influence on the cerebral circulation by either stimulating or inhibiting its vaso-motor centre. Among those which depress that centre are amyl nitrite, nitro-glycerine, cannabis indica, and morphia, and are therefore indicated in the anæmic form. Those which stimulate the vaso-motor centre and are useful in the hyperæmic variety are chloral, chloroform, ether, quinine, guarana, caffein, ergot, cold applications, and galvanism.—Dr Reynolds, *The American Lancet*, Feb. 1886.

SALICYLATE OF SODIUM IN PERICARDITIS.—Dr Samuel L. Abbot, of Massachusetts General Hospital, gives an account of several cases of pericarditis occurring in the course of rheumatic fever, which were treated with salicylate of soda. He holds that the pericarditis disappeared rapidly, and the heart was left in sound condition by the persistent exhibition of the salicylate. The friction sound soon disappeared without any evidence of adhesion or serous effusion remaining. Dr Abbot thinks the treatment should be pushed to the extreme of endurance on the part of the patient, the risk of injury from it being far less than that from unchecked pericarditis.—*Boston Medical and Surgical Journal*, 21st Jan. 1886.

OCCASIONAL PERISCOPE OF DERMATOLOGY.

By W. ALLAN JAMIESON, M.D., F.R.C.P., Extra Physician for Diseases of the Skin, Edinburgh Royal Infirmary; Lecturer on Diseases of the Skin, Edinburgh School of Medicine.

URTICARIA OCCURRING IN INFANCY.—In a communication read before the Clinical Society of Paris, Dr Comby states that Professor Bouchard, in his researches upon dilatation of the stomach, has observed that a certain number of these patients are subject to more or less annoying outbreaks of urticaria. It is not at all surprising that a disease which has for its principal and immediate consequence faulty elaboration of the ingesta should awaken cutaneous manifestations like urticaria, when we see temporary digestive disorder accompanied by the same symptoms. This urticaria, due to lesions or functional disorders of the stomach, is very probably a toxic urticaria, due to the fact that the skin serves as a route of elimination to the poison elaborated in the digestive passages, and taken up into the circulation. Dr Comby tried, in a certain measure, to apply to the infants coming under his observation the results which were obtained from adults by Prof. Bouchard. The dilatation of the stomach, encountered so often among the latter, Dr Comby had found with almost equal frequency among infants of the poorer classes, subjected from birth to defective alimentation. Among animals, also, he had found, as a consequence of dilatation of the stomach, multiple disorders in many of the organs and tissues. The diverse eruptions grouped by authors under the name of *gourmes* (prurigo, scald head, etc.), appear with extreme frequency in little patients with dilatation of the stomach; but of all those eruptions that which had struck him most by its objective and subjective characters is urticaria, of which he reports five cases.—*Philadelphia Med. Times*, 3rd Oct. 1885.

PASTES IN SKIN DISEASES.—Van Hartingen recommends the following modifications of Unna's pastes:—For eczema, Kaolin paste, No. 1.

R. Pulv. kaolini,	
Glycerini, ...	āā 45·00
Zinci oxidi,	
Liq. plumbi. subacet., ...	āā 30·0 M.

This paste forms a fluid like cream. When linseed oil is substituted for the glycerine a soft paste is obtained, which spreads easily. In acne he employs the following:—

R. Sulph. ppt., ...	75·0
Pulv. calcis carbonat., ...	5·0
Zinci oxidi, ...	40·0
Pulv. amyli oryzæ, ...	20·0
Glycerini, ...	40·0
Aquæ, ...	300·0 M.

—*Monatshefte für praktische Dermatologie*, Nov. 1885.

ECZEMA MARGINATUM.—Funk of Warsaw treats this with—

R Acid salicylic,		10·0
Ichthyoli puri,		20·0
Sp. vini rect,		100·0

This is rubbed in with a brush twice a day, and the surface then dusted with powder. He finds this method active, yet gentle, since it does not cause any observable irritation. An extensively-diffused case, which had lasted eighteen years, was so cured in three weeks.—*Monatshefte für praktische Dermatologie*, Oct. 1885.

THE ORIGIN OF CUTANEOUS HÆMORRHAGE.—V. Kogerer remarks that extravasations of blood under the skin are found most frequently in the course of chronic diseases, which markedly interfere with the general nutrition. These are also met with in acute and subacute diseases. In scurvy and purpura, cutaneous hæmorrhage is an essential part of the disease; and the results of Kogerer's researches in these latter corresponded with the morbid appearances found in the first class. As the outcome of numerous sections he found:—1. Disease of the vessels; 2. Thrombosis; 3. Extravasation of blood; and 4. Pigmentation. (1.) There was thickening of the vascular walls; and in order to produce a rupture of these, the weakened state of the surrounding tissues must be borne in mind; but especially (2) the thrombosis. The cause of this is partly general, partly local. A slowing of the blood cannot, in consequence of the lessened contractile power of the heart and possible alterations in the blood, induce it. The exit of blood is favoured by the morbid changes in the vascular walls. These play a double part throughout the whole process, since in the first place they promote the production of thrombi in many cases; and, secondly, the extravasation of blood is facilitated by the plugged vessels and consequent local disturbance of the circulation.—*Zeitschrift für Klinische Medicin*, 3 Heft, 1885.

RECENT ADVANCES IN THE TREATMENT OF SKIN DISEASES.—From a paper in the *Aerztlichen Vereinsblatt für Deutschland*, 1885, No. 158, by Dr Unna of Hamburg, we extract the following:—The value of the sulpho-ichthyolate of soda consists in its high percentage of sulphur, its easy solubility in water, and its absolute innocuousness. It exercises a favourable and local influence on various anomalies of cornification, as in pityriasis simplex, seborrhœa sicca, ichthyosis, and desquamation, after the acute exanthemata, while the ordinary sulphur preparations can only be employed in eczema with caution and constant supervision of their effects. A weak (2 per cent.) addition of the ichthyol salt to the ordinary remedies used for eczema is always advantageous, favours a rapid desiccation, in particular in the freely secreting eczema of children. Still more efficacious is its internal exhibition in all those eczemas ascribed to a nervous origin. The dose for adults is 15 grains, for children 3 to 7 grains daily. Resorcin

possesses the advantage of being without smell over the ichthyol preparations, yet is not so entirely harmless, and thus in universal and chronic skin diseases must be cautiously used. There are some eczemas which present the clinical features of the parasitic class, for which resorcin is pre-eminently the remedy. To these belong a very obstinate eczema of the eyelids, which weeps little, but is frightfully itchy, and readily fissures. This is often combined with eczema of the orifice of the nose and mouth, and sometimes with that of the scrotum. A 10–30 per cent. resorcin ointment or salve muslin quickly cures these. Resorcin is destined to play a much more important part in the therapeutics of skin diseases than it has so far done. The application of water, as hot as can be borne, is of much service in many skin affections, and especially in rosacea. The application must not be continued long, about half a minute, and then the face is exposed to the air, or dried without friction and powdered. A short but hot douche is particularly to be recommended after each meal, or any exposure to heat or exertion. Indeed, in all affections where there is thickening of the horny layer, applications of *hot* water are to be preferred to the use of *cold*. In some cases this is the only possible mode in which water can be employed. The use of powder bags instead of mere dusting is another step in advance. The powder is placed as a thin layer between two folds of a material not in itself too thick, and this is quilted to keep the powder diffused. These bags or cushions are placed over various parts and secured in position, thus permitting a constant fine stream of powder to be directed on the parts. When used, no fat must be previously smeared on. This method is specially useful when we wish to observe how much the conditions present are due to disease or to a too energetic or irritating treatment. The powder cushion ensures rest, and very soon the natural condition of things becomes clear. Among the various pastes an extremely efficacious lead paste, which resembles in colour that of the skin, and is to be recommended in troublesome eczemas of the hands and fingers, is thus compounded by aid of heat:—

R	Amyli oryzæ,	.	.	.	.	10·0
	Lithargyri,					
	Glycerini,	.	.	.	.	30·0
	Aceti,	.	.	.	.	60·0
	Misce. Coque ad	.	.	.	.	80·0

This also acts well in the painful and fissured eczema of the perineum and scrotum. This must be compounded in *small* quantities and freshly. Also an ichthyol paste is a rapidly effective drying application in all varieties of intertrigo, in slight eczemas of the face and hands which do not bear fatty applications, and in extensive superficial eczemas:—

R	Ammoniaë,	
	Sulpho-ichthyolici,	1·0-3·0
	Aquaë,	
	Glycerini,	
	Dextrini,	55 10·0
	Misce leni calore.	

This liniment-like mixture, which rapidly stiffens on the skin, smarts for a minute on eroded parts, but subsequently quiets the itching and pain. These pastes are, like the gelatin and glycerin preparations, permeable to the cutaneous transpiration; and when they dry, cool like powder. In rosacea, whether combined with acne or not, the internal administration of ichthyol preparations is strongly advocated:—

R	Ammoniaë,	
	Sulpho-ichthyolici,	5·0-10·0
	Aq. distill,	20·0

Of this mixture, 15-30-50 drops, gradually increased, are taken night and morning in water. The taste is best overcome by free dilution. A general improvement in health takes place, and the medicine need not be discontinued during the menstrual periods. In lupus the margins of the patches are always difficult to deal with, and Unna lauds an instrument made by Nyrop, Copenhagen, for puncturing and injecting at the same time. As a medicine for injecting, a 50 per cent. ichthyol solution, with or without the addition of tincture of iodine, a 10 per cent. pyrogallic acid, or a 2-5 per cent. solution of caustic potash, are the best. In furunculosis after eczema of the scalp, or in recurring furunculosis of the axilla, rapid cure is effected by the application, twice a day, of a 10 per cent. ichthyol ointment.

PERISCOPE OF SYPHILOLOGY.

By FRANCIS CADELL, F.R.C.S. ED.

THE NATURAL HISTORY AND THE TREATMENT OF SYPHILIS.—The pamphlet from which this abstract is taken is a reprint from *Journal of Cutaneous and Venereal Diseases*, vol. ii., No. 5, May 1884, of an article by Dr Schumacher, junior, resident physician, Aix-la-Chapelle. Dr Diday, of Lyons, for many years has distinguished a decreasing and a progressing syphilis. Against the former he employs hygienic remedies, and avoids specifics; but against the latter he uses mercury or iodine and hygiene. This progressing form is that where so-called tertiary symptoms (gummata), mixed with secondary, appear; one must surely apply specifics, namely, *mercury*. About a discussion on this subject in the Parisian Society of Surgery, 1867, Diday says: "Although mercury cannot prevent the secondary symptoms, it tends to weaken the disease, but it neither prevents recurrences, nor does it destroy

the diathesis; children of syphilitic parents are less liable to infection in course of time." He gives no mercury to patients that have indurated ulcers, or to those who have light secondary symptoms, nor to those who have psoriasis palmaris and moist papules, but does give it to those affected with scaly and pustular exanthemata, or iritis, onychia, and dysphonia. From the history of his cases of syphilis, Diday now suggests five points of controversy:—1. Does mercury, given on the appearance of the chancre, prevent secondary symptoms? The answer is, No. 2. If mercury does not prevent secondary symptoms, does it not at least weaken the further progress of syphilis? Diday answers, that the success of the expectant treatment, and that of the treatment with mercury immediately after the appearance of the chancre, is the same in both cases. 3. Is mercury as powerless against relapses as it is powerless to prevent the first breaking out of secondary symptoms? Although Diday for many years has proclaimed the inefficacy of mercury, and although his cases mentioned in previous years proved now and then the failure of mercury as a curative agent, yet it did not prove this failure to be a general rule. 4. For twenty years Diday has maintained the view that there is a consistency between the gravity of the first general symptoms, that is, of the primary and secondary, and the gravity of the later symptoms of syphilis. The eruptive form (erythematoso) of the first syphilide promises a quicker decline, and is easier to control; syphilis which appears immediately with papulous exanthemata presages, besides a long duration, a deeper infection of the organism, and then iritis, onychia, and dysphonia are to be foreseen. Finally, the squamous and pustular forms are followed by lasting and strong headaches, by persistent sleeplessness, and, after short duration, by purulent processes, impetigo, ecthyma, and rupia. The darker the colour of the first eruption the worse is the prognosis. 5. Diday considers syphilis in most cases a mild disease. 6. Finally, Diday concludes, for the use of the practitioner, that he, without disapproving in the slightest of mercury, only recommends its application in the sphere of its power and its benefits, that is, to soften with it every severe breaking out of syphilitic process. Dr Schumacher's experience is not at all parallel with the views above reported. In Aix-la-Chapelle, in consequence of the peculiarity of the professional work, only difficult cases of syphilis are met with. Diday may by no means be counted among the anti-mercurialists, as he tries to make one believe. In the author's opinion, in syphilis appearing with severe symptoms, the application of mercury in the form of embrocation or inunction of the blue ointment will, above all others, give the physician the best results.

GONORRHOEA IN THE FEMALE.—By Andrew F. Currier, M.D. (concluded from page 693.) The author repeats the statement, that the loose tissue of the vaginal fornix is very commonly a source

of trouble when all other portions of the mucous membrane have been cured. The mucous membrane of the cervical canal may be involved as a primary or secondary condition. Less frequently the urethra and bladder are affected, this being especially true of the bladder. Extension of the disease, by way of the Fallopian tubes to the ovaries, parametrium, and the general peritoneum may occur, and cause fatal peritonitis. A form of the disease, which until recently has been overlooked, has been described by Martineau as that which involves the peri-urethral or pre-urethral glands. They surround or partly surround the urethra near the meatus. Their ducts pass upward within the vestibule, and open on either side of and just under the clitoris. The inflammation which affects them may be acute, or it may be exceedingly chronic, persisting long after all apparent symptoms had disappeared. The gonorrhoeal form is the most frequent affection of the Fallopian tubes. The diagnosis rests on the discovery of gonococci. If these are found in the discharges from the genital passages, and tubal disease is evident, removal of these organs will probably reveal the organisms in the secretions which they contain, or in the mucous membrane itself. The diagnosis of tubal disease by palpation is, however, extremely difficult. In a great many cases sterility is unquestionably the result of gonorrhoea. As regards treatment attention should be given to a wise and judicious prophylaxis. The author has no elaborate scheme to offer, but the remedy lies in the direction of a properly systematized medical police, with suitable hospitals or stations properly equipped. A description is given of all the most potent germicides, with the strengths in which they retard the development of specific germs and destroy their vitality. He has found, in a large number of cases, that the sublimate treatment (that is, by local applications) was quite unsatisfactory. The best results he has seen have been mainly from free applications of a mixture of glycerine and subnitrate of bismuth. Its function consists in a free depletion of the congested tissues, and a very decided astringent action. The proportion of a drachm of bismuth to an ounce of glycerine may be used, the mixture being, apparently, only a mechanical one. Such a mixture may not be directly destructive to gonococci, but, by the modifying action which it has upon the secretions, it deprives them of the qualities which are necessary to cultivation-fluids, and thus the microbes die of starvation. When the symptoms point to the extension of the disease to the tubes and ovaries, the question of abdominal section becomes a pertinent one. He believes the operation is done too often.—*New York Medical Journal*, 24th October 1885.

CASE OF SYPHILITIC ULCERATION OF THE INTESTINE.—The following case is published in the *Lancet*, 3rd October 1885, by Mr A. Blackmore, M.R.C.S.—A prostitute, æt. 25, presented herself at the Manchester Lock Hospital, and was admitted on

15th June 1885, complaining of great pain about the vulva and vagina. These were found swollen, red, and exceedingly tender. A copious sero-purulent discharge was present. She had had syphilis within the last three years. The skin presented numerous copper-coloured patches. The mucous membrane of the mouth and throat was free from ulcers. On the 18th copious hæmorrhage from the bowels took place. The hæmorrhage recurred to a large extent on the succeeding days with but little pain. On the 1st, 2nd, and 3rd of July the bleeding continued, and she sank on 6th July. During the time she had been in hospital her temperature never exceeded $99^{\circ}2$. At the post-mortem, the viscera, to the naked eye, were all fairly healthy. The uterus was intensely congested, with submucous hæmorrhages. The mucous membrane of the small intestines congested; but Peyer's patches not more affected than the other parts. From the cæcum downwards to the middle of the large intestine the mucous membrane was studded with ulcers and small nodules in various stages of ulceration. The ulcers varied in size from a sixpence to a pin's head, with sharply cut edges of a circular form.

Part Fifth.

MEDICAL NEWS.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentlemen passed their final examination at the sittings held on 8th April 1886, and were admitted L.R.C.P. Ed.:—Albert Watson Griffin, Leicester; William Thelwall Thomas, Liverpool; Francis Arthur Vere Denning, Sligo; Roland John Riley, Nuneaton; John Mulvany, Blackburn; David Hamilton, Derry; Robert Usher Russell, Cashel; Alfred Wright, London; Benjamin James Innis, London; Arthur Thorley Wood, Suffolk.

OUR readers and the profession generally will congratulate Dr Keiller on the well-deserved honour of LL.D. he has received from the University of St Andrews.

PURE TEREbene.—We have had forwarded to us by Burroughs, Wellcome, & Co., specimens of their "*Pure Terebene*," and having carefully examined and tried the same, have much pleasure in favourably recommending it to the notice of the profession. From the experiments of Dr Murrel and others, the value of this drug in the treatment of winter cough and other diseases has been clearly established, and we can confidently assert that this Pure Terebene of Burroughs, Wellcome, & Co. is clear, colourless, and of an agreeable odour, and is a most reliable preparation.

LIQ. EUONYMIN ET PEPSIN CO. (OPPENHEIMER'S).—Having been favoured with a sample of this preparation, we can confidently recommend it as an excellent preparation in dyspepsia depending hepatic derangements.

OBITUARY.

JAMES LONGMUIR, M.D.

ON Wednesday, the 24th ult., the inhabitants of Bathgate and the surrounding district were startled with the intelligence that Dr Longmuir had died very suddenly. A native of Airdrie, on obtaining his degree at Glasgow University he settled down in Bathgate some seven and twenty years ago, following Dr Barry Dow in the conduct of the practice of the late Dr Dickson. During all these years he has gone in and out among the people, and there is little wonder that his sudden removal has caused a blank which will not readily be filled up, as his courteous and genial manner made him many friends. Amid the arduous duties of a country doctor's life he found time to devote a considerable share of his leisure to local politics, being a Justice of the Peace for the county, for many years a Commissioner of the Burgh, and also serving as Chairman of the School Board for three years. As a mark of their esteem and regard the magistrates resolved that the funeral should be a public one, and accordingly, on Saturday the 27th, the shops were closed, and a large number of the inhabitants followed the cortege to the grave.

Dr Longmuir is survived by his second wife and seven children, the youngest only a week old at its father's death.

THE LATE DR WILLIAM N. BROWN, MELROSE.

A WIDE but (*cheu*) swiftly thinning circle will read with regret that this much respected medical practitioner died on February 14th, in the 71st year of his age. Palsy of a sharp character, following on a long, harassing course of distressful, intracranial disease, was the immediate cause of death. It is comforting to know that when his troubles were about to cease loved ones were near, and that the end was peaceful. He was the second son of the late Rev. Dr Brown, of Broughton Place Church, Edinburgh, and was born at Biggar, a little town situated among the sequestered uplands of South Lanarkshire. Brought to the metropolis in 1822, when his father was translated thither from Biggar, the deceased was in due season put through the usual classical curriculum of the High School—an Institution at this time predominated over by Dr Carson, one of the most erudite classical scholars of his day—and was thereafter apprenticed to the late Mr Syme, at this period a highly popular extramural teacher of surgery at Minto House Hospital. There can be no doubt that the field laid open to him at the Surgical Hospital had tenfold greater charms for him than had the gerund-grinding of the High School; and it may be safely said that seldom, if ever, was gentle knight more loyally served by courteous squire than was the singularly gifted surgeon at the head of the former establishment by his young, enthusiastic, leal-

hearted pupil. Ardent and eager in temperament, the deceased did not fail to take ample advantage of the marvellous teaching power which at this time (1830-40) characterized the Metropolitan Medical School, intermingling with his professional work, now and again, sundry exhilarating pedestrian excursions through the beautiful mountain-ranges of the Scottish Border, together with an occasional ramble by the lakes of Cumberland. The laureation season came in 1837, when he had to defend his inaugural thesis on "Morphology,"—a topic, without doubt, suggested to him by his keen relish for botanical science in all its departments. A short time after having taken his academical degree he chose the pleasant little town of Melrose as the seat of his professional labours—a position from which he never shifted till struck down by what ultimately proved to be a mortal malady. As a practitioner Dr Brown was skilful, sympathetic, and candid, doing his best on all occasions to steer a safe middle way between the healing conservative forces of Nature and the time-tried appliances of a reasonable empiricism. With the poor patient, who is always with us, he was full of ruth and discreetly generous. Practice of the nimious, showy sort he looked on with deep scorn. As regards professional ethics, his creed might have lain in a nutshell, founded as it was on principles delivered long ago in a glowing pastoral charge sent to a little flock at Philippi. As a friend—though, peradventure, at times a little impulsive—he was leal, steadfast, and very lovable. Need it be said, in fine, that to the few who are here who shared with him the toils and pleasures of student life, and saw him facing bravely the professional and domestic cares of middle age, his absence leaves a saddening gap which time can hardly be expected to repair? Dr Brown leaves behind him a bereaved widow and a family of several children.

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- A. L. GALABIN, M.A., etc.,—*A Manual of Midwifery*. J. & A. Churchill, Lond., 1886.
- FRED. J. GANT, F.R.C.S.,—*Science and Practice of Surgery*. 2 vols. Baillière, Tindall, and Cox, Lond., 1886.
- A. W. GERRARD,—*Elements of Materia Medica and Pharmacy*. H. K. Lewis, Lond., 1886.
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From the "British Medical Journal," Feb. 14th, 1885.

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III.—UPPER EXTREMITY.

32. *Amputations of the Hand.*—In partial amputation of the

DIRECTIONS TO THE BINDER.

EDINBURGH MEDICAL JOURNAL.

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in One Volume, or in Two Parts.

THE HAND; AVOID A CIRCULAR IN THE PALM. A CIRCULAR IN THE PALM
is apt to be tender, and this interferes with the grasping power of
the hand.

In amputating a finger do not interfere with the breadth of the
hand. In a case requiring removal of one or more metacarpals
leave, if possible, healthy periosteum; new bone is formed, and a
more useful hand is the result. Let this rule regarding the peri-
osteum hold good, very specially in connexion with the meta-
carpal bone of the thumb. Any osseous projection at the radial
edge of the hand is a point of attachment for the muscles of the
ball of the thumb, and is of the greatest use as an opposing point
to the fingers.

In patients in whom manual labour is their source of income,
do not, in amputating the fore and little fingers, interfere with
the heads of the corresponding metacarpals, if a sufficient covering
can be obtained. In other cases, for the sake of appearance, the
head of the metacarpal may be removed obliquely.

Take, if possible, your main flap in amputating any of the fingers from the flexor aspect of the finger. Do not approach the palm in your incisions. In the middle and ring fingers the best result—looking to use and not to appearance—is obtained in the following way: Enter the knife at the knuckle, carry it outwards and forwards towards the web until a point midway between the anterior and posterior aspects of the web is reached. Do the same on the other side of the finger; these two incisions form a right angle with each other. A flap is then made from the anterior aspect of the first phalanx. The finger is removed, and the flap is turned back into position, the apex of the flap fitting into the angle where the incisions begin over the knuckle. By this method the incisions do not approach the palm, the breadth of the hand is not interfered with, and the resulting cicatrix is posterior.

In crushes of the hand save as much as possible; save a finger or a portion of a finger; save any part of the thumb; save any portions of the metacarpals. The most useless natural hand is more useful than any artificial substitute.

33. In *contractions of the palmar fascia* Busch's operation in severe cases affords the best result. In simple cases the subcutaneous division of the tense fibres is generally sufficient. It is to be remembered that there are two directions in which the contracted fascial fibres must be divided, parallel to the skin surface and at right angles to the skin surface; by the first, the fibres at right angles to the skin surface, which dip down between the flexor tendons, are divided; by the second, the longitudinal fibres of the contracted palmar fascia are divided.

Busch's operation consists in dissecting the contracted fascia from the flexor sheaths by a V-shaped flap, the apex of the flap looking to the wrist; the fingers are then extended, and the flap attached with horse hair stitches to the edges of the incision, while the opposing edges of the proximal portion of the raw surface are accurately stitched together. The result is a Y-shaped cicatrix, and an extended finger or fingers with no tendency to subsequent contraction.

34. In *wounds of the palm* the persistent hæmorrhage is often due to the palmar vessels being simply punctured, and not cut fairly across. Divide the artery wounded by deepening the accidental wound. Retraction of the wounded vessel takes place, and simple pressure is sufficient to arrest the hæmorrhage. Check the force of the blood flow by fully flexing the forearm on the upper arm with a pad at the bend of the elbow. By these means the hæmorrhage is arrested; if it still persists, plug the wound in the palm; if this fails, tie the brachial artery.

35. *Whitlow*.—In deep-seated digital inflammations over the first and second phalanges, the cause is either an inflammation of the flexor sheath, or it may have a periosteal origin. In inflammations over the anterior aspect of the terminal phalanx, the cause

is periosteal, and the worst that can happen is necrosis of the terminal phalanx.

In all cases make your incision early, central and in the long axis of the finger. Relieve tension, and prevent spread of the inflammation from the flexor sheath on the finger to the common flexor sheath on the anterior aspect of the wrist. In periosteal cases early incisions prevent necrosis of the affected phalanx. Whitlows are infective conditions, and are due to a colony of micrococci. The periosteal whitlows are cases of acute suppurative periostitis.

The infective character of these conditions must not mislead the surgeon into supposing that careful antiseptic precautions are unnecessary. Sepsis and infection are perfectly distinct conditions. Relieve the tension, and the evil effects of the pathogenic micrococci will soon subside; prevent sepsis caused by the entrance of septic organisms from the external air, and rapid healing will be the result. In patients who are liable to whitlows, as in people who suffer from boils and carbuncles, administer corrosive sublimate internally, it is a most powerful anti-fermentative.

In *inflammation of the common flexor sheath* relieve the tension by making an incision into the sheath in the forearm above the annular ligament. Take care and not injure in your incision the median nerve; adopt Hilton's method to avoid this risk. After opening the flexor sheath in the forearm, pass a curved probe-pointed bistoury from the wound under the annular ligament, divide it with the knife, and in this way the palmar tension is effectually relieved.

36. When called on to perform *amputation* for injury or disease in the upper extremity, do not follow, at the cost of length, any special method of amputation; get your flaps as best you can, so as to obtain as long a stump as possible. The longer the stump the easier it is to fit on an artificial substitute. In severe injuries of the upper extremity in which an endeavour is made to save the limb, more especially in cases in which the bones are injured, in which the line of fracture is oblique, or in which, from comminution of the bones, it is difficult to keep the fragments in accurate position, remember that the use of the extension apparatus is as valuable in the upper as it is universally acknowledged to be in the lower extremity. Thick sheet lead makes a most efficient splint, it can be easily moulded to the injured limb over the dressing; by its weight it steadies the limb and keeps it at rest.

37. In all *fractures* near joints the soft tissues are to a certain extent saved from injury when the bone gives way, but still in all cases there must be some injury to the tendons, muscles, joint, and ligaments. These structures require, for the proper performance of their functions, mobility; prolonged rest to prevent any risk of non-union of the fractured bone, may be followed by stiffness of the neighbouring joint, by adhesions of the ligaments, and organized

effusion into the sheaths of the tendons. The result is a united fracture with a stiffened joint.

Non-union of bone does not occur in consequence of occasional gentle passive movement along with massage, if in the intervals the parts are kept at perfect rest. Non-union is much more likely to occur if slight constant movement is allowed between the broken ends. For example, in fracture of the shaft of the humerus, and in fracture of the shafts of the radius and ulna, it is important to keep the elbow-joint at rest by means of a rectangular splint. If the elbow-joint is not kept quiet, there is more or less *constant* movement at the seat of fracture. This movement is very different from gentle passive movement every second day, with perfect rest in the intervals, as in fractures in the region of the wrist, elbow, and shoulder.

In Colles' fracture allow the patient to move his fingers and thumb after the first week, and after ten days take off the splints every second day and move the fingers, thumb, and wrist-joint gently. Take off all splints at the end of 4 weeks. Too prolonged rest in this injury often ends, more especially in old people, in irremediable stiffening of the fingers, thumb, and wrist-joint.

In fractures into the elbow-joint early gentle passive movement at the end of a fortnight every second day prevents stiffness of the elbow-joint.

In fractures of the upper extremity of the humerus begin passive movement after a fortnight.

In *dislocation* of the thumb backwards at the metacarpophalangeal joint, dorsi-flexion of the thumb, with pressure on the head of the dislocated phalanx, is the simplest way to treat the case. In dislocation of the fingers the extension is best made by means of a toy made of plaited strong grass so arranged that it can be easily slipped over the finger, but when it is pulled upon it grasps the finger tightly.

Fractures of the third and fourth metacarpals are diagnosed with difficulty. They are best treated by an anterior splint. Oblique fractures of the phalanges are most troublesome. It may be necessary, in such cases, to apply extension. An anterior splint, carefully padded so that there may be no pressure on the ball of the thumb, stretching from the bend of the elbow well beyond the tips of the fingers, is fixed to the forearm by sticking-plaster. An elastic band is attached to the injured finger by sticking-plaster, and extension is kept up by fixing it to the extremity of the anterior splint.

In fracture of the phalanges utilize the neighbouring fingers as lateral splints, padding carefully between the fingers so as to prevent discomfort, excoriation, and itching. Skin should never be allowed to remain for any length of time in contact with skin. In fixing the arm to the trunk in fracture of the clavicle and in

fracture of the upper extremity of the humerus, if a layer of lint is not placed between the arm and the chest much discomfort will follow.

In these fractures it will generally be found that the broken ends of the fractured bone are best brought into apposition by bringing the arm well across the chest, so that the hand lies on the opposite shoulder.

In fixing the arm the use of a long strip of sticking-plaster fixing the limb to the trunk is a simple way of treating these injuries. In green-stick fracture of the clavicle, a common accident often overlooked at the time of the injury, the strip of sticking-plaster is the best method of treatment.

In fracture of the clavicle at the coraco-clavicular ligament there is no displacement. In fracture of the clavicle external to the coraco-clavicular ligament there is no downward displacement, and the forward displacement is not observed at the time of the fracture, but becomes very evident at a subsequent date. Treat all fractures by simple means; let wood, pasteboard, and lead (in cases in which the patient is confined to bed) be your mainstays; avoid all special forms of apparatus.

In *sprains*, carefully applied elastic pressure, with wadding, combined with massage and passive movement, gives the best results.

In diagnosing an injury look before you touch the limb. Remember the normal relations of the styloid processes in diagnosing injuries in the region of the wrist; the relation of the head of the radius to the external condyle, the relation of the olecranon to the internal condyle of the humerus in the elbow-joint; and let the coracoid process and its relation to the head of the humerus be the principal guiding landmark in injuries of the region of the shoulder.

Always expose the uninjured corresponding region, examine it in the first instance, and let it be your standard (having satisfied yourself that it is normal) in diagnosing the injury on the opposite side.

IV.—LOWER EXTREMITY.

38. In amputations of the toes, a partial amputation may be performed in the great toe; in the other toes partial amputations are inadmissible; avoid any incision in the sole of the foot. Remember that the foot is a tripod, and that its stability rests on the integrity of three points of support—the ball of the great toe, the ball of the little toe, and the os calcis; interference with any one of these lessens the value of the foot as a basis of support. Any narrowing of the foot approximating the two anterior points of support also renders the foot less stable.

Utilize the plantar surface for the principal flap in amputations through the tarsus and at the ankle joint. In amputation at the tarso-metatarsal joints and in amputation through the centre of the

tarsus, after marking out the flaps by incisions down to the bones, it is best to disarticulate and then dissect the bones off the long plantar flap from behind forwards.

In all amputations in the lower extremity sacrifice length in order to obtain a stump that will bear pressure. A painful stump is worse than useless ; with it the patient has no comfort, and cannot wear an artificial support.

In amputation above the ankle the long anterior flap gives the best result. In amputations below the knee the modified circular is, as a rule, preferable to the long posterior flap. If the latter method is adopted a posterior leaden splint, curved so as to support the long posterior flap, is the best means of preventing retraction. In all amputations the posterior leaden splint is the best steadier of the stump. Lead as a splint, from its weight and plasticity, makes an excellent splint in many injuries, and after operations both in the upper and lower extremities.

In sawing the bones in amputations in the leg always enter the saw upon both bones at once, so that the fibula may be divided before the tibia. In amputation below the knee it is often difficult to secure the arteries. When such difficulty arises take a curved needle, threaded with catgut, and pass it into the tissues behind the bleeding point so as to include the tissues around the vessel in the ligature.

In Carden's, Gritti's, and Spence's amputations in the region of the knee-joint take care not to make the posterior flap too short. The resulting cicatrix always tends during consolidation of the stump to pass backwards, because the posterior group of muscles are divided further from their origins than the opposing muscles on the front of the thigh.

In amputation for injury through the shaft of a long bone the periosteum may be divided at a lower level than the bone ; if this is done it is best to save the periosteum on the anterior surface of the bone, and allow a flap of periosteum to hang over the divided medullary cavity. Do not stitch it for fear of deep-seated tension.

In amputation at the hip-joint amputate by the circular method below the trochanters, tie the vessels, turn the patient round so that he lies on the uninjured side, make a vertical incision over the trochanter, keeping well back where the vessels are not important and the trochanter is most superficial, and disarticulate the head of the bone.

In all amputations for injury, in which the patient has lost much blood, save any blood escaping at the time of the amputation, and mixing it with a 5 per cent. solution of phosphate of soda, as described by Mr John Duncan (*Ed. Med. Journal*, April 1886), inject it into the main vein before stitching together the flaps.

39. *Ulcers* are due to a local or constitutional cause ; in most cases the local cause is the direct excitant, the constitutional cause rendering the patient more liable to evil consequences from

the local irritation. Unless in the case of a burn or other distinct traumatic cause, always be suspicious of a constitutional cause if the ulcer is situated on any part of the body except the lower half of the leg, and even there be suspicious if the ulcer is on the posterior aspect of the limb. Ulcers are prevented from healing either by a congested or an indurated state of the limb. Simple rest in the recumbent posture, elevation of the limb, and careful elastic pressure are the indications for treatment under which painful, foetid, and spreading ulcers will, with few exceptions, become painless, sweet, and clean. Improve the vitality of the soil, and the putrefactive organisms will die out, not finding a suitable nidus for their further growth and development. The use of antiseptics, such as iodoform and chloride of zinc (40 gra. to the ounce), is of secondary importance to an improvement in the vitality of the limb. They are, however, very valuable as adjuncts to the elevation treatment. After the ulcer has assumed a healthy appearance, if the patient must go about, apply elastic pressure *before* the patient rises from bed. This is a most important point, which possibly Dr Martin was the first to insist upon.

40. When a patient is brought under your notice with pain in the knee, for which you cannot find any evident local reason, always carefully examine the hip; and in a patient who limps as if from hip-joint disease, if you do not find in the hip evident objective symptoms of joint disease, always carefully examine the back. He may be suffering from vertebral disease, with effusion into the psoas muscle under the psoas fascia.

41. In fractures of the leg use the box splint—two pieces of wood rolled in a sheet. See that the foot is kept at right angles to the leg, and thus retraction of the heel is prevented. Take care that there is no eversion of the foot. In oblique fractures use extension.

In fractures of the patella fix to the anterior aspect of thigh a large piece of sticking-plaster, and make through it extension on the quadriceps extensor cruris—elevating the limb on an inclined plane with a foot piece.

In fractures of the thigh use extension with the weight and pulley, take care that the weight is not too heavy, and measure the limb every third day, so that the weight may be reduced. The too prolonged use of the weight may result in delayed union or in non-union. In children, in restless adults, and in cases of delayed union, use a double long splint with a transverse cross piece. In other cases a single long splint is sufficient; with the double long splint the patient is fixed in a wooden box, so that he can only move his arms and his head.

V. SYPHILIS.

42. If common diseases are the most important, then this disease and its results are among the most important. Taught by

Spence that soft chancres and syphilis are due to one poison, taught by Syme that all the evils of syphilis are due to mercury, and knowing that these doctrines are still held by many surgeons, I am glad of this opportunity of stating my firm opinion that there are two distinct poisons, and that mercury is of immense value in the treatment of syphilis.

Syphilis is an infective fever; a specific organism is planted on an abraded surface, it lies quiescent for a time, and germinates if the soil is suited for the growth of the organism; if the soil is unsuited the growth is aborted, but I have no evidence that this aborted form is the poison which gives rise to a soft chancre.

It germinates locally; it passes into the blood stream through the lymphatic circulation; in its passage along the lymphatic vessels it is arrested in the lymphatic glands and there germinates; it is also arrested in the capillaries of the skin and mucous membranes, and there gives rise to various local lesions, the different early and superficial skin and mucous membrane lesions. The poison may get a firm hold of the individual either in consequence of the virulence of the poison or in consequence of the suitability of the soil, and then gives rise to evil sequelæ or bad consequences, often called tertiary symptoms.

These sequelæ do not necessarily follow, and are becoming less frequent: *1st*, because the poison is becoming less virulent; *2nd*, because the poison is not now finding so suitable a nidus as it once did. These two factors interact on one another; the syphilitic poison is becoming less virulent because both factors are at work; the country is becoming gradually syphilized, the soil is being gradually worn out, the poison is thereby gradually being weakened. To grow a plant and to get good seedlings you require a healthy vigorous plant and a good soil; if the soil is worn out the seedling, however strong, does not thrive; if the seedling is weak, the soil, however suitable, does not favour its vigorous growth and development.

At one time mercury, given in large doses until the patient was poisoned, so weakened the individual that, although the secondary symptoms might be masked, and apparent temporary good results followed, no permanent good resulted, and tertiary symptoms were liable to occur. Nowadays the sequelæ are principally observed in weaklings—weaklings either from hereditary transmission, or from acquired feebleness, the result of evil courses persisted in by the person himself after the poison had entered his system.

Syphilis is gradually wearing itself out, but still the administration of mercury in small doses is of value. The patient will tell you that he feels the better of it, that it acts as a tonic. The explanation of this feeling of wellbeing is probably a simple one. Syphilis is a fermentation, a zymotic disease; the fermentation is due to the growth of an organism in the tissues. Corrosive sublimate has been proved to be a most powerful anti-fermentative.

Give it in doses which will not be poisonous to the individual, but poisonous only to the organism of syphilis, and the growth of the organism will be checked, the strength of the individual is not impaired, and gradual elimination of the poison will take place, because, like all zymotic conditions, it has a tendency to a natural cure. The disease may undoubtedly, taking advantage of this tendency, be treated without mercury; by attention to the general health, by keeping the individual at a high level of vitality the poison may gradually disappear from the system, by opposing to its growth and development the anti-fermentative action of the healthy tissues; but even in healthy people I am satisfied that mercury is of value, and my rule is to give it from the very beginning, and continue it for a year or eighteen months from the beginning of the attack. The feebler the individual the greater is the necessity of a mercurial course.

The diagnosis of syphilis is founded on the period of incubation. If, after a suspicious connexion, a period beyond ten days elapses before any local manifestation is evident, then the probability is that we have to deal with a case of syphilis. In such a case begin the mercury at once. In making your diagnosis do not be deluded by a local manifestation, either in the form of a gonorrhœa or a soft sore, either of which appear shortly after connexion. The patient asks, "Have I syphilis?" How often is the answer given, "No," in one sense true—true of to-day, as far as any one can tell, but not necessarily true three weeks hence. Your answer should be, "I cannot tell; you *have* a gonorrhœa; you *have* a soft sore, but the time has not yet arrived to enable me to say that you *have not* syphilis. Six or eight weeks must elapse before I can speak with any certainty." The patient is not satisfied; this you cannot help.

Another snare to the unwary practitioner is that the patient always tends to blame the *last* connexion. This connexion is probably the cause of the gonorrhœa or soft sore, while the patient may have been inoculated previously with the syphilitic poison, which may at the time you see him be lying quiescent, and become evident at a subsequent date. At the end of the first week the gonorrhœa or soft sore becomes painfully evident; the average period of incubation in syphilis is three weeks; if two connexions follow one another at an interval of less than a fortnight, it is evident that when the patient comes to you with the gonorrhœa or soft sore you cannot speak of his freedom from syphilis, which may show itself soon after the gonorrhœa, if caught from the first connexion; if caught from the second connexion, it may not show itself for a fortnight or three weeks after you are consulted.

The hard nodule, typical of syphilis—not a sore, not an ulcer, but an elevation, a local hypertrophy at the seat of inoculation—shows itself from ten days to eight weeks after connexion. The average

period of incubation is, in my experience, three weeks. This typical local hypertrophy may ulcerate if it is irritated in any way, but ulcerative destruction of tissue is an accident, not an essential of the primary lesion. This fact alone seems to me to go a great way in itself to show that the poison which causes a soft chancre—an ulcer—is different from the poison of syphilis. I cannot say how they were related in the past; I cannot say what may be their relations in the future. Neither an antiquarian nor a prophet, but simply dealing with the conditions as they come before the practical surgeon at the present time, I have no hesitation in saying that there are two distinct poisons—the one giving rise to a local ulcer, the other to a constitutional condition; the one probably more nearly allied to a putrefactive poison, the other to a truly infective poison; the one septic, the other pathogenic.

The typical syphilitic hypertrophy may be masked if the poison has effected its entrance on the urethral mucous membrane, it may also be masked if the poison has its seat on a soft sore; the latter is more common than the former. These cases do occur, and often deceive the practitioner. They are, however, no evidence that the two poisons are one. There is, in my opinion, no logical resting-place between accepting the belief, held by John Hunter, and disproved by Benjamin Bell, that gonorrhœa, soft sore, and syphilis are one disease, and the belief that there are three poisons, often met with in one and the same person at one and the same time. It will surely not do to argue that because gonorrhœa is very rarely followed by syphilis, therefore these conditions may be considered as separate diseases, but because syphilis not unfrequently follows a soft sore, it is therefore more likely that these two conditions are one disease. May not these clinical experiences be simply explained by the gonorrhœal poison thriving in a different habitat from the syphilitic and soft sore poisons; that the latter have the same habitat is no proof that they are the same plant. The position is, in my opinion, untenable logically, untenable clinically.

In concluding, for the time being, these demonstrations, I almost feel that an apology is required. They deal with such common things, and they are so well known to many, that I fear they are hardly worthy of a place in the *Edinburgh Medical Journal*. My excuse is, that perhaps I have directed attention to some practical points which I have learned by experience and by profiting from my mistakes. At any rate, they are sufficient to illustrate the position insisted on in the first of these lectures, that the simple common diseases and injuries are worthy of consideration, and that ignorance regarding them is more fatal to success in practice and to personal comfort than ignorance regarding the rarer and more serious surgical diseases and operations.

II.—CASE OF OPERATION FOR STRANGULATED HERNIA WITH GANGRENOUS OMENTUM, AND RADICAL CURE.

By JOSEPH BELL, F.R.C.S. Ed., Surgeon to the Royal Infirmary, Edinburgh.

J. C., æt. 52, a muscular, thick-set man, was sent to me by Dr Gilmour, of Linlithgow, in the following state:—Patient had a left inguinal hernia five years before, but it had not again protruded, and he had neglected his truss. During a severe exertion a large hernial protrusion came down on the morning of 23rd February 1886, giving intense pain and rapid symptoms of strangulation. Taxis was used with energy for one hour without chloroform without success. Next day the doctor again used taxis under chloroform for nearly two hours without succeeding in reduction, and in the evening sent the patient to hospital. When I saw him the case looked very unfavourable; pain was intense; vomiting was constant; the patient was collapsed, and covered with clammy sweat. The hernia, as large as a child's head, was of a deep purplish-black colour, exceedingly tense, filling the scrotum; the penis was also dark and discoloured; and the tissues were brawny with serum. I operated at once, and found the cellular tissue dead, the sac bruised, and in it two coils of small intestine; one about twelve inches, the other about nine inches in length, of a purplish-black colour, but still retaining lustre, polish, and tension; the internal ring was very tight, and there was a mass of omentum, dark, engorged, and evidently with gangrene commencing, which overlapped the intestines. After drawing down the loops, and finding their vitality entire, I divided the ring, and returned them, but the omentum was evidently hopeless. After drawing down an unbruised part, I tied it in three parts with chromicized catgut, leaving the ends long; cut off the bruised part. I then brought the neck, thus secured, into the ring, and sewed it and pillars of the ring together with catgut, bringing the ends of the ligatures, which had been left long, out at the lower angle of the skin wound. I brought it also deeply and firmly together with catgut sutures. The operation was conducted throughout under irrigation by bichloride of mercury solution (1-2000). The patient never had a bad symptom. The cellular tissue came away without putrefaction or suppuration, and now, on 2nd April, he has a perfectly solid, firm ring and canal, the stump of omentum having become united in a thick, fibrous mass. As a measure of precaution, he is to wear a truss for a few months, but there is not the slightest impulse on coughing at any part of the ring.

III.—CASE OF INTRA-TRACHEAL TUMOUR.

By R. MACKENZIE JOHNSTON, M.D., F.R.C.S. ED., and JOSEPH M. COTTERILL, M.B., F.R.C.S. ED.

(Read before the Edinburgh Medico-Chirurgical Society, 7th April 1886.)

LAST summer, while I was in temporary charge of the Ear and Throat Department of the Royal Infirmary during the absence of Dr M'Bride, a patient came to me from the country for advice, suffering from what I found to be an intra-tracheal growth. As the case is one of some interest and considerable rarity, I think it right to place it on record. The following are the notes I made of the case, and which I publish with Dr M'Bride's permission:—

M. R., aged 13, a school-girl, came to the Ear and Throat Department on 14th September 1885, complaining of occasional suffocative attacks and considerable difficulty in breathing, more especially at night. She stated that she had not previously suffered from any serious illness, but, on the contrary, had been particularly healthy. Two years before she had a severe inflammation of the throat, but no details of any importance could be elicited about this, nor could she suggest any possible cause for her present condition. There was no rheumatic, phthisical, nor syphilitic taint in the family. Two sisters had died in infancy from childish diseases, but in every other respect her family history seemed to be unexceptionable.

Her mother gave the following account of the commencement and course of the present illness:—About twelve months ago she first began to notice that the child experienced some difficulty in breathing and occasional spasmodic choking attacks. The latter gradually wore off, but the breathing became worse, till she sometimes thought that the child would be suffocated at night. At this time also there appeared a certain amount of hoarseness or roughness of the voice; and this was, to a slight degree, perceptible when I first saw her. There was never any difficulty in swallowing, and she had neither cough nor pain. During the past months she had been under various forms of treatment. She had blisters applied to the throat, she had used several inhalations, and she had had medicines for bronchitis or asthma, but without deriving any benefit from their use.

On examination I found the patient to be a strong, healthy-looking girl, and well grown for her age. There was no tenderness nor swelling to be discovered in the neck. Her breathing was audible at some little distance, owing to a slight inspiratory stridor, but the dyspnoea was not distressing. She could phonate quite distinctly, but apparently only with some little effort.

On inspection with the laryngoscope I found the glottis widely open, and the epiglottis elevated, so as to give a full and excellent

view of the larynx and its cavity. The cords were normal, and quite free in all their movements. Looking beyond the cords, however, I saw a small roundish mass, about the size of a cherry-stone, situated in the trachea, apparently about 1 inch below the cricoid (in reality it was found to be more than $1\frac{1}{2}$ inch below). This tumour seemed to block up almost the whole lumen of the canal, and to be in contact with the posterior, anterior, and left surfaces of the trachea, the point of attachment being the former. On the right side the air-passage seemed reduced to a narrow slit, but doubtless the space was greater than appearances would lead one to suppose. There was no evident movement of the tumour even during forced respiration, from which I assumed that it must be sessile. In shape it was regular, smooth, and nearly spherical, and in colour it was rather paler than the surrounding mucous membrane. Its appearance varied a good deal at subsequent examinations, but probably this was due to the quantity of mucus covering it, or to the varying intensity of the reflected light employed to illuminate it. I passed a probe into the trachea with the view of ascertaining the consistence of the growth and its attachments and actual position; but this brought on such violent spasm, that I was obliged to desist from further attempts. An exact diagnosis was not possible, owing to the situation of the tumour and the difficulty of examining it thoroughly; but the case was evidently one of intra-tracheal neoplasm, either of a fibrous or polypoid nature.¹ I recommended early tracheotomy for the removal of the tumour, and advised the mother to allow the child to remain for that purpose. She was admitted to Dr Duncan's wards, then under the charge of Dr Cotterill, who arranged to operate on the following day. The removal of the growth was successfully performed by Dr Cotterill; but as he has kindly written a short account of the operation, I need not further refer to it here. On 6th October, about three weeks after the removal, I again examined the patient with the laryngoscope, and found the trachea quite clear; but on the posterior aspect there was a small greyish nodule, which might be either a slough or a recurring tumour.

9th October.—The disappearance of the nodule showed the former supposition to have been the correct one. The patient was free from all signs of obstructed breathing, and as the tracheal wound had quite healed, she was pronounced fit to return home. At the end of December (more than three months after the operation) I examined the patient, and found that she continued well, and perfectly free from her former symptoms.

Remarks.—Stricture of the trachea may be the result of extrinsic or intrinsic causes. Intrinsic stricture, or occlusion stenosis, may

¹ Since the above was put into type the tumour has been examined by Dr Bruce, who reports that its structure is that of a fasciculated sarcoma with an investment of epithelium, consisting of several layers of dentate cells.

be produced in various ways, and one of these, as illustrated in the foregoing case, is by the growth of neoplasms from the inner surface of the trachea. Although the larynx is a comparatively common situation for morbid growths, the trachea seems to be but seldom invaded by them. So rare, indeed, are intra-tracheal tumours, that Morell Mackenzie¹ states that, in his extensive throat practice, he has only met with four examples of this disease, while in sixteen years he has operated on 223 cases of laryngeal tumour.² Again, in von Ziemssen's exhaustive *Encyclopædia*³ there is no special mention of growths in this situation further than a passing allusion to them as an occasional cause of tracheal stenosis; and in other standard works I find no reference to them at all. Mackenzie explains the comparatively rarer occurrence of the latter growths by pointing out that the trachea is less liable to irritation, being a smooth passage, whose functions are merely passive, while the larynx presents numerous irregularities, and its component parts are in constant motion. To Solis Cohen, of Philadelphia, we are indebted for most of our information about such cases. He has, with great care, collected and tabulated⁴ all the examples of this disease that have been recorded up to the present time—amounting, in all, to about 45 cases. The large proportion of these that were not discovered till after death makes it probable that many more may have escaped notice where the cause of death was not cleared up by a post-mortem, or because there is not the same desire to publish a case when the diagnosis has not been correctly made during life. Thus, of the 45 cases referred to—18 were diagnosed by laryngoscopy, 17 were diagnosed at the autopsy or during tracheotomy, and in 10 no information is given on this point. The records of many of these are so meagre, and the total number is so small, that it is almost impossible to generalize about them, but still we may note one or two points by a careful comparison of them. Adults, it would appear, are more often attacked than children, and men than women. Any portion of the trachea may be the seat of the disease; but the membranous part is the usual situation, and the relative frequency becomes less the further down the air-passage we go. As to the etiology of these growths we know nothing. After tracheotomy granulations, sometimes of considerable size, have been observed to appear on the cicatrix, and these, if large, may give rise to very similar symptoms; but they must not be confounded with the histologically true neoplasms at present referred to. They are called post-tracheotomic vegetations, and have been specially studied by Petel.⁵ In none of Cohen's

¹ *Diseases of the Throat and Nose*, vol. i. p. 524.

² *Idem*, p. 299.

³ *Encyclopædia of Med.*, v. Ziemssen, vol. iv. p. 479.

⁴ Ashhurst's *Encyclopædia of Surgery*, vol. v. p. 743.

⁵ *Des polypes de la Trachée*, Paris, 1879.

tabulated cases were the tumours connected with a former tracheotomy. Almost any form of non-malignant growth may occur in the trachea, but fibromata and papillomata seem to be the most frequent, 10 examples of the former and 8 of the latter having been recorded. Polypi, enchondromata, lipoma, cysts, etc., have also been observed. Besides the benign tumours referred to above, several examples of malignant growths have been recorded by Shrøtter, Rokitansky, and Klebs; but these, though of much interest clinically, have no bearing on the present case, as they are generally, though not always, secondary to disease elsewhere, and are not usually amenable to the same treatment.

The most important and chief symptom of this disease is dyspnoea—its intensity depending on the size and rapidity of growth of the neoplasm. Irritation of the trachea producing spasm may exist, and in my case this was noted at an early stage; but it passed off, possibly owing to the canal becoming accustomed to the growth. The prognosis is undoubtedly serious, fully 50 per cent. of the known cases having terminated fatally.

A diagnosis in this disease can only be made with certainty by means of a laryngo-tracheoscopic examination. This is often difficult or even impossible to perform; but, with practice, it may be often managed, for Mackenzie says he is now able to see the bifurcation in 40 per cent. of all cases examined. In M. R.'s case the examination was facilitated by the wideness of the glottic space, and the upright position of the epiglottis. In another case of obstructed breathing that I can recall, I noticed a similar condition of the parts. May this not be due to increased action of the abductors or to extra muscles being brought into play in the struggle to get air into the lungs? It would be interesting to know whether this corresponds with the experiences of others.

There can be little doubt that, with few exceptions, tracheotomy should form the first step in the treatment, both because it is the safest for the patient and the most thorough in its results. A few cases, however, have been treated by endo-laryngeal operation, but this is only possible when the tumour is situated high up. Labus succeeded twice in removing the growths by forceps, and Mackenzie¹ succeeded in two out of three cases in obtaining a cure with the electro-cautery. Shrøtter² got rid of one growth by the injection of ferric chloride, but in other hands the same plan in one case terminated in sudden death. In Cohen's table I find that tracheotomy was proposed to 10 patients, but was declined by 4, while in the remaining 6 the operations were successful, and resulted in cure, except in one case, where diphtheria supervened after four days, causing death. All the four cases not operated on eventually terminated fatally.

¹ *Op. cit.*, vol. i. p. 525.

² Cohen, *op. cit.*, p. 748.

ACCOUNT OF THE OPERATION.

The extreme rarity of benign tumours affecting the trachea renders it advisable that any operation performed for their removal should be carefully noted; but I do not propose to detain you by a lengthy consideration of the operation performed in this case of tracheal tumour; but rather, in a short sketch of the case, to touch upon one or two points which appear to be of practical interest.

The patient, M. R., aged 13, came under my care upon the 14th of September last, while I had charge of Dr Duncan's wards in the Royal Infirmary. During the three days which she spent in the Infirmary previous to the operation I had several opportunities of examining the tumour, and agreed with Dr M. Johnston (who had sent her to me for operation) in considering the growth to be a fibroma: sessile; almost spherical in shape; blocking up almost the whole of the lumen of the trachea, leaving only a small crescentic chink in front and to the right through which the air passed, and attached, as far as I could judge, at least an inch and a half below the larynx, posteriorly and to the left side. The dyspnoea was very considerable at times, especially at night; and due precautions were taken in case of any suffocative fit supervening, which might render tracheotomy necessary.

And here a question presents itself. In a case of this sort, where for various reasons the removal of the tumour is not to be undertaken for a day or two, how far is one justified in delaying the performance of tracheotomy? Should I have performed it at once on her admission, and then proceeded to the removal of the tumour subsequently? I believe not, and for the following reasons:—

1. The tumour was sessile and motionless, and consequently no sudden change of its position could have intensified the dyspnoea. In the case of a movable pendunculated growth or loose foreign body in the trachea, the matter is entirely different, and no delay is admissible.

2. The worst fits of dyspnoea had always occurred at night during sleep; they were evidently due to the collection of mucus in the narrow chink between the growth and the tracheal wall. These fits of dyspnoea she had always easily dispelled on waking by coughing, and we were able to help her to this end by placing her in a suitably warmed and moistened atmosphere.

3. It is unwise to insert a tube unless you are prepared to go on with the operation of removal of the tumour at the same time, for reasons which will appear in the narrative of the case.

Upon 17th September the patient was put under the influence of chloroform, and I proceeded to expose the trachea by an incision beginning at the lower margin of the thyroid cartilage, and extending downwards some 4 inches towards the sternal notch.

The superficial parts being drawn aside, there came into view an enormously enlarged thyroid gland. It was of a firm and fleshy consistence, and absolutely devoid of any appearance of an isthmus, its shape resembling that of a very large chestnut. It closely covered the trachea from the cricoid cartilage for at least an inch and a half downwards.

At this stage of the proceedings the patient vomited, and some of the vomited matter lodging in the air-passages caused such alarming symptoms of suffocation (breathing stopping entirely, and the patient becoming rapidly and deeply cyanosed), that I was forced to open the trachea as quickly as possible wherever I could get at it. I accordingly cut through the cricoid and first ring of the trachea, and passing in a feather cleared the passage, and had it held open with blunt hooks till the patient recovered herself.

Here I would remark that in such circumstances to hastily put in a tracheotomy tube would have been worse than useless, as it would have injured the tumour, and by causing it to bleed, or breaking off a piece of it, might have made matters worse. I think it is an essential that in all such operations the tube should be put in *below* the tumour or not at all. (Of course I exclude such cases as where tracheotomy is done for spasm of the glottis due to a tumour situated low down and out of reach of the tracheotomy tube.)

Through the opening that I had now made into the trachea I was able to satisfy myself that the tumour, as we expected, was situated on the posterior wall and about an inch below my incision into the trachea.

I then set to work to clear the trachea below the enlarged thyroid gland; and this was a matter of great difficulty, owing to the distended veins and the very deep position of the trachea, as I had to open it very low down on account of the enlarged thyroid gland, and in order to avoid the tracheal growth. Having inserted a large well-fitting tube through the lower wound into the trachea, I then removed the fibroma through the upper opening with a pair of polypus forceps curved on the flat, keeping the patient's head well down over the end of the table in order to prevent any blood running into the lungs in case hæmorrhage from the growth should prove considerable. There was not, however, very much bleeding, and it soon stopped with the pressure of a small pad of lint.

It has been recommended in these cases that the stump of the growth should be thoroughly cauterized with nitrate of silver. I did not do this, however, but scraped with the finger-nail the stump of the growth until I was satisfied that it was thoroughly eradicated.

The tumour, which I show you, is a fibroma of firm consistence. It has shrunk considerably in the preserving fluid since its removal. It measures $\frac{7}{8}$ of an inch long, $\frac{1}{8}$ broad, and about $\frac{1}{2}$ of an inch deep.

The tube was removed upon the second day after the operation. I was anxious to get it out as soon as possible, as it was necessarily of large size, in order to reach the deep-seated wound in the trachea, and so fitted very tightly; and it lay in such close proximity to the aorta, as to be very visibly moved by every pulsation of that vessel.

After the operation the breathing was always quite easy, and upon the day after the removal of the tube she could phonate quite distinctly.

Ten days after the operation the wound in the neck had closed, and the day following she got up and walked about the ward. She left the Hospital on the 10th of October cured, having been previously examined in my absence in the country by Dr McBride and Dr Johnston, who reported that the throat was to all appearance normal.

I examined the patient a short time ago, and found that no trace whatever remained of the tumour; and though five months have elapsed since the operation, there is no appearance of any return of the growth. She breathes and phonates normally, and sleeps without any noise. Her general health seems to have improved considerably since the removal of the growth. She was exhibited at the last ordinary meeting of this Society.

It is, of course, impossible to generalize from one case, but the following inferences may perhaps be drawn from a consideration of this case taken in conjunction with the reports of other similar cases given by Störk, Gibb, Morell Mackenzie, Fifield, Türk, and others:—

1. That sooner or later these cases tend to destroy life by suffocation, unless some operation for their removal is resorted to.

2. That when the tumour is small and high up in the trachea, it may in some cases (as in one reported by Dr Morell Mackenzie) be cured by the application of the galvano-cautery, with the help of the laryngoscope; though in one of three cases reported by Dr Morell Mackenzie he reports that the vocal cords were injured by the cautery. Dr Mackenzie also states that three out of the four cases which he has seen were papillomata and not fibromata.

3. That when the tumour is low down in the trachea, large, and of firm consistence, or when the patient is not a good subject for laryngoscopy, the best proceeding is one similar to that which I have described.

4. That, unless the tumour is very high up in the trachea, the first step of the operation should be to open the trachea *above* the tumour. One can then judge exactly by sight or touch of its precise position and nature, and ensure the second and lower opening for the introduction of the tube being made accurately below the tumour. The tumour can then safely be removed through the upper opening, while the lower opening gives one an opportunity of plugging the trachea and preventing hæmorrhage into the lungs.

5. That when a tube can be inserted below the tumour, the growth is best removed by forceps or otherwise at once; but that when a tube cannot be put below, it may be safer to attack the growth through the tracheal wound with galvano-cautery. This method obviates the chance of bleeding which, though not excessive in my case, would certainly have been troublesome, if I had not had a tube in below the site of the tumour, and was so enabled to employ pressure by a plug of lint to the bleeding stump.

It also obviates the risk of injuring the vocal cords with the cautery, as happened in one of the recorded cases.

6. That the subsequent history of this case shows that the application of caustics to the stump of the tumour is probably uncalled for.

IV.—THE ANALGETIC ACTION OF CARBOLIC ACID AND THE CRESOL GROUP OF COMPOUNDS.

By ROGER McNEILL, M.D. (Edin.), Resident Medical Officer, Gesto Hospital, Edinbane.

THE advantages of local anæsthesia have of late been brought into prominence, owing to the discovery of the action of cocaine. To be able to operate without causing pain, and without the necessity of taking away consciousness, or otherwise endangering the life of the patient, would undoubtedly be the perfection of anæsthesia. All that can be said as yet is, that this is a consummation devoutly to be wished.

My object in writing this paper is to give a short description of a few experiments performed by myself on myself, with the view of finding out the action of carbolic acid and the cresol group on the skin.

Dr Erasmus Wilson (*Journal of Cutaneous Medicine*) and Dr Bill (*New York Medical Journal*) have directed attention to the anæsthetic action of carbolic acid in 1870. According to Dr Smith (*New York Medical Journal*), 85 per cent. solution of carbolic acid produces local anæsthesia. On applying that percentage of the acid in olive oil to the skin of my left forearm, I found that all sensibility to pain was lost in a very short time. It, however, caused a very sharp feeling of painful heat, which lasted for a few minutes, and left a white mark slightly raised above the surrounding skin. This afterwards inflamed and suppurated. With the view of discovering the possibility of taking away the feeling of pain without causing the unpleasant after effect, I made the following experiments:—

Carbolic acid (pure) was mixed with olive oil, with glycerine, and with melted vaseline in the proportions of 30, 40, 50, 60, 70, 80 per cent. of the acid with the other diluents. The acid formed a mixture with the oil, which separated on standing. With the

glycerine it seemed to dissolve and continue in solution. The vaseline separated almost entirely on cooling, and was therefore given up as an inconvenient form.

On applying the mixture of the carbolic acid in olive oil to the skin of my left forearm, no effect was produced after repeated applications until the strength of 50 per cent. was reached. That mixture diminished the sensation to pain slightly. After repeated applications 60 per cent. of the acid in olive oil caused almost immediately a feeling of heat, followed by a white corrosive appearance of the skin and loss of sensation to pain. I have at different times made a cut through my skin, about an inch long and from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch deep, with the aid of this mixture without feeling any pain, while the cut healed kindly without any dressing. If, however, the deep fascia of the arm was touched pain was felt. The loss of sensation to pain was more marked with this percentage of Calvert's No. II. than with the pure deliquescent crystals, and with these crystals than when absolute phenol was used.

On applying the percentages stated of the acid in glycerine to the skin of my forearm, no feeling of heat was produced, and the sensation to pain was not lost until the strength of 80 per cent. was reached. A slight flush was all that was produced by the applications below that strength. Absolute phenol appeared to be weaker than the pure crystals, and these than Calvert's No. II., as was the case when mixed with olive oil. The fact that a higher percentage of the acid was required when mixed with glycerine led me to believe that glycerine might take away the irritation of the acid when mixed with olive oil. I accordingly applied 60 per cent. of the acid in the oil to my skin. When the feeling of heat appeared at its height, and the white corrosive appearance was produced, I applied pure glycerine to the part. It almost at once stopped the heat, and the corrosion soon disappeared, while the part remained insensible to pain. On repeating the experiment several times, it was found out that if the glycerine was applied before the whitish appearance was fully developed, it took away the feeling of heat and the corrosion, but at the same time prevented analgesia from being fully developed. If, however, the corrosive action was well marked, the analgesia continued for some time afterwards. The feeling of heat was slight and transitory, but the corrosion was more marked, and liable to end in suppuration. Glycerine prevented this.

The fact that in the case of carbolic acid its analgetic action diminished in proportion to its purity, led me to believe that this power might be owing to something mixed with it. I therefore determined to try the effect of benzol, toluol, and cresol, these agents being prepared from coal and closely related to carbolic acid in their composition. Benzol and toluol had no effect whatever on the skin. For a long time I was unable to get a sample

of the cresols. A fluid called cresolene was advertised as a powerful antiseptic. On getting a sample of it, I mixed it in the same proportion with glycerine and olive oil as I did in the case of carbolic acid; and on applying it to my skin, I found that it was quite as strong an analgetic as carbolic acid, and appeared to have been less corrosive. Through the kindness of Professor Crum Brown of Edinburgh University, I found out that Kahlbaum of Berlin prepared some of the cresols. That chemist sent me four bottles. One contained a dark fluid with a tarry odour, not distinguishable in colour or smell from the cresolene above alluded to. This was stated by Kahlbaum to be a mixture of ortho- and para-cresols, and to be prepared from toluidine. Another bottle contained a fluid of the colour of pale sherry, with a sickly smell, and stated to be prepared from tar oil. This fluid was a mixture of three isomeric cresols (ortho-, meta-, and ortho-cresol), and is called theerol. A third bottle contained pale orange crystals, called para-cresol, fusing at 30° C., and boiling at 188° C. The fourth bottle, pale crystals, called para-cresol,—fusing point, 36° C.; and boiling point, 198° C.

On wetting the skin of my forearm with a few drops of the toluidine cresol, slight heat was felt in a short time, the skin turned white, and analgesia was complete over the area covered by it in two minutes. Sensation to pain returned in about an hour. The cuticle was raised into a blister on the following day.

Theerol was painted in a similar manner on another spot. There was an immediate feeling of painful heat, and white corrosion gradually appeared. Sensation to pain returned in about an hour. The cuticle raised in the form of a blister in about forty-eight hours, and suppurated.

Some crystals of the ortho- and para-cresols were applied to different places at the same time. They melted and acted on the skin in a similar manner to the fluids.

Toluidine cresol was then mixed in the proportions of 40, 50, 60, 70, 80, and 90 per cent. with glycerine, and in the same proportion with olive oil. After the application of the 60 per cent. in olive oil, sensation continued normal for ten minutes, when it was rubbed off; but on applying it in the proportion of 80 per cent., I was able in three minutes to transfix a hypodermic needle in the part without feeling any pain. There was no feeling of heat caused by it, and there was no appearance of corrosion. A slight flush was left over the area covered by it. A cut was then made without causing any pain, but pain was felt when the subcutaneous tissue was reached.

Theerol was found to render the skin analgetic in a few seconds in the proportion of 70 per cent. with olive oil. There was a slight transitory feeling of heat, but no corrosion beyond a slight reddish flush, which disappeared during the night.

Para-cresol, in the proportion of 50 per cent. with olive oil,

rendered the skin analgetic in a few minutes. There was a slight flush left, but no heat nor any inconvenience. Ortho-cresol in the same proportion produced no change. Having had only a small sample of the ortho- and para-cresols, I have not yet been able to make further experiments.

Sensation to pain was scarcely if any affected by theerol in glycerine until it reached the strength of 90 per cent., and at that strength I had to repeat the application several times before the analgesia was complete.

In all the above experiments, although no pain was felt on cutting the skin, the sensation to touch was intact or but slightly affected. The least scratch or contact could be felt, but no pain, a fact which goes to prove that sensation to pain and to touch are distinct—that is, that pain is not exaggerated touch. According to Schiff, this condition may be induced by a division of the whole spinal cord, with the exception of the posterior columns, or by rapid abstraction of blood. It has also been noticed during the early stage of general anæsthesia.

In order to find out the effect on the sensation of the subcutaneous tissues, I tried the following experiments on the middle finger of my left hand. In order to prevent bleeding, and also with the view of diminishing the chance of absorption of the agent used, I tied a tight indiarubber band round the middle of the third phalanx, after applying 80 per cent. carbolic acid in olive oil to the skin at that point. This prevented any discomfort from the tight band being felt. I tried weaker mixtures than 80 per cent. without avail, probably owing to the acuteness of the sensibility of the finger, or perhaps from the epithelium being harder and thicker. I then painted a line on the back of the second phalanx with 80 per cent. cresolene in olive oil (weaker mixtures had no effect). This, however, rendered the part unsensitive to pain. The skin was then cut through, and the wound deepened gradually until pain was felt. The wound was then opened, and 60 per cent. cresolene in olive oil dropped into it. This produced analgesia of the deeper tissues, and enabled me to cut down until I approached the extensor tendon, where I deemed it advisable to stop. The skin sloughed where the band was tied, and the wound ulcerated and inflamed, and I was invalided for several days; but this was partly owing to want of care, and exposure to the cold of a frosty winter night within the following twenty-four hours. The tight band probably diminished the blood supply, and therefore the vitality, thereby enhancing the corrosive action of the carbolic acid. I afterwards applied 50 per cent. cresolene in olive oil to my forearm, and pressed on the spot for some time, and found that it acted more powerfully than would otherwise be the case.

I may now give a short description of a few cases on whom I
1 some of these agents to facilitate operation.

A man, aged 80, was admitted into the Hospital with epithelioma of the lip. I painted a V-shaped line inside and outside the lip, and cut the piece away, causing but very little pain, according to the statement of the patient. It healed kindly for the most part by first intention. 60 per cent. carbolic acid in olive oil was used.

A nervous woman of 60 was admitted suffering from a small fibroid tumour on the back of her leg above the ankle-joint. She would not have it cut without chloroform. One day I made her lie on her face in bed, and painted the skin over the tumour with 60 per cent. cresolene. I then injected 15 minims of cold water containing 1 minim of liq. morph. hypodermica under the tumour. An oval incision was then made, and the tumour was dissected away without pain. A small drainage-tube was introduced, and the wound stitched. The woman stated that she did not know that her skin was cut, although she felt as if I was rubbing the part. The wound healed by granulations, more slowly than might be expected.

A boy, aged 14, was admitted suffering from contraction of the plantar fascia of the right foot. 80 per cent. theerol in olive oil was painted over an area about an inch square of the instep, where I intended to cut the fascia. The above strength was used, as the epithelium was thick. Analgesia was complete in about two minutes. Fifteen minims of cold water were then injected subcutaneously over the fascia. The knife was then passed through and under the skin, and its edge turned towards the fascia, without causing any pain. The boy stated that he felt pain when the fascia was stretched and being cut with the knife. It healed without any dressing but cotton wool.

Several cases of abscesses have also been opened with the aid of the above agents without causing pain.

The conclusions, therefore, which may be drawn from the above experiments and observations are—(1.) That the agents mentioned produce analgesia when applied to the skin; (2.) That mixed in certain proportions with glycerine or olive oil, they take away the sensation of pain without causing any untoward effect, either locally or constitutionally; (3.) That the strength used must vary according to the thickness of the epithelium, and perhaps the acuteness of the sensation; (4.) That certain operations may be performed by their aid painlessly without chloroform; (5.) That they are corrosive when applied to the skin, unless sufficiently diluted; (6.) That glycerine has more power in preventing corrosion by these agents than olive oil, and might probably be administered with benefit in cases of poisoning by carbolic acid or any of the above agents.

V.—NOTES ON FOUR CASES OF AINHAM.

By D. G. CRAWFORD, M.B., C.M. Ed., Surgeon, I.M.S.

SEVERAL cases of this uncommon and interesting disease having come under my notice during the past year, I thought that a few notes on the subject might be not without interest. The only countries in which, as far as I know, this disease is met with, are Brazil and Lower Bengal. In the former, it appears to be very common; in the latter, while not very rare, it cannot by any means be considered common.

For the last thirteen months I have acted as resident surgeon of the Medical College Hospital, Calcutta, and in that capacity it has been part of my duty daily to see the surgical out-patients. During these thirteen months the total number of out-patients attending, each patient being counted as only one unit no matter how long he may continue to attend, has been 12,535, or a little over 1000 a month. In this time I have met with four cases of ainham, and, including a case which was seen by my substitute while I was away on ten days' leave, five cases have been met with, or rather less than 1 in every 2500 surgical out-patients. Of these 12,535 patients, 2042 were Europeans and Eurasians; 10,493 natives. Excluding the former class, in whom ainham, I believe, has not been noticed, the proportion remains rather less than 1 in 2000.

The fullest account of the disease with which I am acquainted is to be found in a work entitled "A Report on Certain Endemic Skin and Other Diseases of India and Hot Climates generally," compiled and published under the sanction of the Secretary of State for India, in 1876, by Dr Tilbury Fox and T. Farquhar. During 1872 the Duke of Argyll, then Secretary of State for India, ordered five hundred copies of a "Scheme for Obtaining a Better Knowledge of the Endemic Skin Diseases of India," to be circulated officially to medical officers stationed in India, while the Director-General of the Naval Medical Department sent further supplies of copies of the same scheme, officially, to medical officers stationed in China, Japan, Egypt, Algeria, Norway, Sweden, Canada, the West Indies, Honolulu, and elsewhere. The work above referred to was the outcome of the answers received after the circulation of this scheme.

Ainham is one of the diseases referred to. Answers with regard to ainham appear to have been received from only six places in India, viz., from the province of Oudh, from Balasore, Hughli, Patna, and Dakka, four districts in the province of Lower Bengal, and from Malabar, in the Madras province; also from Colombo in Ceylon, and from Swatow in China. From all of these the answer was the same, that the disease was unknown there, except from Dakka, the civil surgeon of which district, Dr James Wise, reported two cases. In the Report, however, is reprinted (from the *India*

Medical Gazette) a paper by Dr Alexander Crombie, then resident surgeon of the Medical College Hospital, and now civil surgeon of Dakka, in which six cases are collected,—four from Dakka, one from Noakhali, a very malarious district in Lower Bengal, lying south of Dakka, and one from Calcutta from the Surgical Out-Patient Dispensary of the Medical College Hospital. The only other case which I have been able to find reported, by searching the files of the *Indian Medical Gazette* from its commencement to the present year, is one contributed in 1877 by Surgeon C. J. H. Warden, then civil surgeon of Bhagalpur.

The disease is fully described in the paper by Dr Crombie, reprinted in the Report, and already alluded to above. From this paper I quote the following description:—

“We have not been able to obtain any clue to the primary cause of the disease, but it seems to be essentially manifested in hypertrophy of the skin of the little toe near the digito-plantar fold, and of the tissues surrounding its middle and distal phalanges, with an attempt at spontaneous amputation of the member at or near the first inter-phalangeal articulation; and in the most advanced cases in conversion of the bony structure of the phalanges into fibrous tissue. In these cases the toe presents the appearance of a soft, rounded, or ovoid mass, of about the size of a large marble, attached to the foot by a short, narrow pedicle, which allows of motion in all directions, and the patients apply for relief on account of the deformed toe being loose, and getting in the way of walking. In the early stages the bones are not diseased, and the middle becomes affected before the distal phalanx. The disease is usually symmetrical, but the two little toes do not present the morbid changes in the same degree of advancement. In one of the four persons the disease existed only on one side. The changes take place very slowly. In my own case the disease had existed, according to the man's statement, for fifteen years, without the middle phalanx having undergone fibrous degeneration. The patients do not exhibit any constitutional disturbance or deterioration of health, and the disease is not accompanied by pain, except in those cases in which ulceration round the pedicle occurs, when there is a good deal of suffering; and in one case this was accompanied by œdema, redness, and pain on the dorsum of the corresponding foot. The fifth digit is the only one affected; none of the other toes have shown the least tendency to similar changes.” In another case, the Noakhali one, Dr Crombie found the fourth right and fourth and fifth left toes affected.

The cases which I have seen were the following:—

CASE NO. I.—Puran, a Hindu male, aged 22, a native of Kattak, in Orissa, has lived in Calcutta for the last four months, and for the last month has suffered from ainham of the left little toe. He was offered admission to hospital, for the purpose of having the toe

amputated, but said he would prefer to have it done in the out-patient room, and to go home afterwards. He wished the toe taken off because he found it get in his way when walking, knocking up against stones, etc., as he had no control over its movements, and thus causing him great pain. The toe was amputated at the metatarso-phalangeal joint on that day (29th November 1884) and dressed. He was then taken away by his friends, and did not return again for treatment.

CASE No. II.—Shaik Doman, a native of Sylket, Mussulman male, aged 33, came to the dispensary on 15th January 1885, complaining of ainham of the fourth left toe, from which he had suffered, he said, for about one year. There was no history of injury; he stated that it came of itself. There was a very deep constricting ring round the base of the toe affected, much more marked on the plantar than on the dorsal aspect of the toe. This ring was deeply incised by two deep cuts in the long aspect of the toe, one on the plantar and one on the dorsal aspect. These incisions had completely healed on the twelfth day. The ring appeared to have been broken through on the back of the toe, where there did not seem to be any constriction remaining, but on the under side, where the constriction had originally been deepest, little or no good was done, the ring being still well marked. The patient, however, expressed himself satisfied, and said that he thought the disease was cured, which I very much doubt. He did not again present himself at the dispensary.

CASE No. III.—Abdul, a Mussulman male, aged 30, born in Calcutta, who had been at sea as a lascar for eight years, but had left that employment for work on shore a year ago, came to the dispensary on 24th February 1885 suffering from ainham of the right great toe. The disease, he stated, was of six years' standing, and he thought that it had been caused by the fall of a piece of wood on the toe. The toe hung by a narrow pedicle, the size of a small goose quill. The toe was amputated about a quarter of an inch on the proximal side of the pedicle; the knife, in cutting through the toe, did not encounter any bone. The wound had healed on the fifteenth day, 11th March.

CASE No. IV.—Dorso, a Hindu male, aged 24, a native of Kattak, in Orissa, who had lived in Calcutta for the last eight months, was sent to the Surgical from the Medical Dispensary during the month of April 1885, suffering from ainham of the right little toe, which he had only noticed for the past fifteen days. He refused to have the toe taken off, or to allow it to be incised, saying he wished some medicine to rub on it to cure it. On being told that the disease could not be cured in that way, he went away, and did not come back again.

Remarks.—There was no appearance of symmetry in any of

these four cases, only one toe in each subject being affected. Out of the four, two were in the right and two in the left foot. Only two cases were in the little toe, one in the fourth, and one in the great toe. In the latter there was a doubtful history of injury, but I do not think that any stress need be laid upon that. Little reliance can be placed on the description which an ignorant native gives of how a disease began, and a recollection of the injury happening to the toe, which afterwards became diseased, would be quite sufficient for him to consider the two facts as cause and effect. For the same reason, statements as to the duration of a disease are of little worth. The duration in these four cases was said to be six years, one year, one month, and fifteen days respectively, but in the last two cases at least, and probably in the third also, it must have been greatly understated. The case which had lasted for six years appeared to have come very near to spontaneous amputation. Two cases came from the Kattak district, one from Calcutta, and one from Sylket. The two first districts are in the province of Lower Bengal. Sylket is also geographically in Lower Bengal, but politically in Assam, having been transferred from Bengal to that province eleven years ago.

In Quain's *Dictionary of Medicine* it is stated that early division of the constricting band cures the disease. The case in which I tried this treatment was not very successful. Though the patient expressed himself satisfied with the result, I do not think much good was done. It is not, however, possible to draw any conclusions as to the effects of treatment from one instance. Besides, possibly the disease might not have been treated early enough in the progress of the case.

In the same article it is stated of ainham, that "it is peculiar to the African race, being found not only amongst the inhabitants of the west coast of Africa, but also amongst the Hindus of African descent, as well as amongst the slave population of South America." Whether there is any strain of African blood in the population of Lower Bengal I cannot say for certain, but I should think it is highly improbable, and I never heard anywhere else any suggestion of such a pedigree.

VI.—CASE OF FEIGNED DISEASE.

By W. KYD AITKEN, L.R.C.S. Ed., etc., Surgeon to the Chinese Engineering and Mining Company, North China.

S. W., æt. 33, deputy-overman, reported himself as having been injured by a fall, while on duty in the pit, early on the morning of 10th October 1884. He fell backwards, and hurt his back just over upper part of sacrum and lower lumbar vertebra.

History and Former Illnesses.—He is most eccentric and hysterical, and imagines himself to be suffering from all sorts of obscure dis-

eases. About two months previous to the present accident he had a slight attack of dyspepsia, for which he was treated with rhubarb and soda. At that time I noticed that his tongue was protruded with a jerk and drawn to right side; his face is also drawn to the same side, and he has a consequent strabismus of left eye. He was not aware that there was anything peculiar about his tongue when protruded; and he never had any sickness in his life; had always taken very great care of himself, as he had studied anatomy and physiology, and invariably stopped eating before he felt that he had had enough.

He now states that at the age of 16 he was severely injured. had his arm broken, and several other severe bruises; also about a year ago, after a fall, had an attack similar to the present, when the medical attendant applied a liniment to his back, which, he says, "was very sore, and brought it all out in little blisters." This had a wonderful effect, and he was able to return to his work in about two or three days.

Twenty years ago, he says, he had an attack of "brain fever after over-study for the ministry;" passed, and was appointed to "take charge of a circuit" (Methodist), but on account of his health he did not do so, and preferred to stay working in the pit. His age, according to his own statement, is only 33 now, though he looks quite ten years older. He is a preacher in connexion with the Salvation Army. On the day previous to his accident he was severely reprimanded by one of his superiors for not attending to his duty.

On his way out, in May 1884, he remarked to several men that, if the place did not suit him, he would show them a "new way of getting home again."

Present Illness.—On examination at 11.30 A.M., 10th October, he said, "I wanted to get up to a nail, and I stood on a piece of wood about one foot high. This gave way, and I fell on my back; at the time it did not hurt, and I went on with my work, and it was not till trying to get out of bed about an hour ago that I felt the pain, and found that I could not walk." No bruise or mark of any kind to be seen to indicate the seat of injury. Complains of intense pain in back, shooting up to head and eyes, and inability to walk. No pain on pressure. Pulse and temp. normal.

Treatment.—Rest in recumbent posture, flannel binder, hot fomentations, and arnica.

16th October.—During the last few days there has been no improvement; he has become gradually worse and worse. Corrigan's button cautery applied over seat of injury. Appetite good; bowels regular; temp. and pulse normal.

17th October.—Great pain in head and eyes shortly after application of cautery; numbness in legs, and increasing inability to walk; very strong in his desire that I would not apply the cautery any more.

18th October.—Pain still present. "Fancies" he can walk a little better. Lin. crotonis rubbed over the lower lumbar and sacral regions; eyes examined by ophthalmoscope, and no abnormality discovered. Sensation partly lost in feet and legs, as seen on application of needle point.

19th.—A number of small papules have come out over the place where the croton was applied. Says "back feels much weaker." Walks with legs thrown forward and body back, similar to woman in last months of pregnancy. Pulse and temp. normal; bowels regular.

20th.—A second application of lin. crotonis. Pain still present in back and head over occipital and parietal regions; none in eyes.

21st.—Back pretty well covered with papules. Pain in head and eyes; none in back. Patella reflex present; little sensation except in sole of foot. Temp., 98°·4; pulse, 75; bowels regular.

22nd.—Lin. crotonis again applied. No better; head very painful, but sleeps well, and has good appetite; has wild thoughts and wanderings.

24th.—Lin. crotonis. Just the same; wishes to go home; says he does not wish to be hard on the company, and will cancel his agreement for a free passage to London and his wages (£17 per month) till he reaches there. Has wild thoughts and dizziness in head; no pain in back. He says that on the evening of 22nd he thought he was going to die.

26th.—Lin. crotonis. Had a little diarrhoea yesterday; this morning he says he passed two pieces of clotted blood, also a streak of blood in sputum; neither could be produced when asked for. Nothing could be detected in lungs. Pulse, 88; temp., 98°·4; bowels twice yesterday.

On my recommendation to Mr Kinder, C.E., engineer-in-chief of the works, S. W. was sent to the French Hospital, Tientsin, to be under the care of Dr Frazer, Admiralty surgeon. He was accompanied by one of my assistants, a young Chinaman educated in America. During the journey, 1½ day, he displayed remarkable powers of eating, and also of being able to move about his legs in a most surprising manner during sleep.

While at Tientsin he developed very marked hysterical symptoms, and gave his razor to the nursing sister to keep for him, as he felt tempted to commit suicide.

6th November.—Dr Frazer's report received, in which he confirmed the opinion I had already formed and given to the officials, that S. W. was shamming. This decision was also shared in by Dr Irvine and Dr Mackenzie, of the Viceroy's Hospital, and also the surgeons on board H.I.G.M.'s gunboat "Nautilus."

All the above notes were taken at the time of visiting.

Remarks.—This was an extremely puzzling case indeed, though

it seems easy enough when committed to paper. There are a few points to which I should like to call attention.

1. *His Appearance*.—This was of a man at least 45 years of age though he stated he was only 33; he was a man who has evidently suffered much either from sickness or other troubles; his hair, black, was rapidly becoming white; this change was also shared in by his beard. He expressed great astonishment that one should take any notice of his drawn or puckered appearance of right side of face or the tongue being jerked out and drawn to right side, or of the strabismus—to him it seemed the most natural thing in the world.

2. *His Eccentric and Hysterical Manner*.—This was very marked and gave every one who saw him the impression that there was something wrong. On one or two occasions, when some of his friends called to see him, and especially if they sympathized with him, he would go into a "fit," which, from the description I received, must have been purely hysterical; if any one suggested that I be sent for, he at once became calm, and said he felt better. He had "wild thoughts and wanderings," and also occasionally "clutched" at invisible things. While at Tientsin Hospital, and under constant supervision of experienced sisters, he developed still more hysterical symptoms, especially when any missionaries called with whom he was very popular.

3. *His Inability to Walk*.—This became gradually worse and worse, and his mode of walking like that of a pregnant woman. When he thought no one was near, and during sleep, his movements were not in any way restricted. This was also noticed during his residence at the French Hospital. Total absence of pain on pressure, and on the application of a needle point his muscles contracted, but he did not move, and persisted in saying that he felt nothing; and if the point was firmly applied he said he felt "as if a fly was walking over his leg." There was no difficulty with his urinary organs.

4. *The Decision given*.—When I recommended his removal to Tientsin I gave it as my opinion that he was feigning illness, and added that I should like to have the advice of my friend Dr Frazer, as the case was difficult, and more especially as it meant that the whole future course of the man's life was at stake. Dr Frazer, recognising the difficulty of the case, called into consultation all the other members of the profession to be had, and they unanimously united in the same opinion. I may also add that in this the British Consul concurred.

He was consequently discharged from the company's service, and on arrival at Shanghai posed as a martyr, and a number of missionaries and other benevolent but misguided people subscribed a considerable sum, and he returned to England a comparatively wealthy man, and is now going about perfectly hale and strong, with no sign or symptom of ever having had any injury.

VII.—CASE OF SCIRRHUS OF ABDOMINAL ORGANS IN A YOUNG CHILD.

By A. JAMES DUNCAN, M.D., L.R.C.S.E., Consulting Physician to the Dundee Royal Infirmary.

(Read before the Forfarshire Medical Association, January 28th, 1886.)

CANCER in the young subject is of such rare occurrence, that the record of a case lately met with may, I think, prove of interest.

Joseph H., æt. 3½ years, child of a mill employé, was first seen on November 29th, 1885. For about ten days the mother had noticed that the child felt heavier, but as he continued to play about and to take his food fairly well, she was not, in spite of some fulness of the belly and legs, concerned about him, till the preceding day, when the swelling of the legs markedly increased. I found very considerable œdema of legs and penis. Abdomen was much distended, and tympanitic on percussion all over, except at left iliac region towards groin, where there was some dulness and a good deal of pain. Liver dulness could not be defined. There was some pain in micturition. Pulse 80; temperature normal; respiration hurried. Examination of chest showed absolute dulness of lower two-thirds *right* chest posteriorly, *not* extending laterally or anteriorly, with absence of breath sounds. By next day the œdema was largely gone, and it did not return. Urine was found to be free of albumen.

December 1st.—Pain in lower abdomen to left side was greatly complained of—this was so far relieved by poultices. In another day or two a defined firm fulness, with dulness on percussion, was felt in that region, extending towards bladder. The urine was being passed in bed, but so much did the swelling simulate a distended bladder, that on Dec. 3rd I was prepared to pass a catheter, when on renewed percussion there was clear resonance over pubis, though still dull in left iliac region. Careful repeated examination satisfied me that there was little, if any, fluid effusion in abdomen. The respiratory distress continued and increased, and was often extreme during the night; the symptoms being unrelieved by medication, viz., purgatives and diuretics (potass acetate). On Dec. 6th the right chest was tapped posteriorly between sixth and seventh ribs, and 8 ounces of turbid serous fluid, loaded with lymph flakes, drawn off. Percussion note over dull area of right chest was clear immediately after the tapping, but by next day the dulness had recurred, and never altered in its extent, except, perhaps, rising slightly higher. The diuretic mixture was changed for a combination of caffeine and convallaria. The symptoms being in no wise relieved by the tapping, three days later it was repeated, and 11 ounces of darker serum, not so full of lymph flakes, removed. By this time there was also established a dulness over lower third left

back. Neither did the second tapping give any relief; the dyspnoea continued as severe, especially at night. R. 48, P. 120 to 140.

December 12th.—Meeting a medical friend I took him to see the case, as I was somewhat in doubt as to its exact nature. I drew his attention to a firm swelling in *right* flank, between ribs and crest of ilium, which had been noticed a day or two before, and also to the presence of crackling over the seat of pain, and fulness in *left* iliac region. He thought there was considerable ascites, and urged tapping of abdomen. The trocar of an aspirator was accordingly introduced with but little result, and not much more when the pump was attached. During next day or two, however, a constant oozing from the puncture rendered the abdomen less tense, and admitted of fuller palpation. The liver was then felt below the ribs, and its dulness was apparently continuous with that of swelling in *right* flank already mentioned.

December 15th.—Dyspnoea previous night had been very urgent, so aspirated *right* chest for third time, and drew off 6 ounces of still darker coloured serum. The child's aspect was now decidedly pinched. The appetite, which all along had been bad, was quite gone. A small knot was noticed in the *right* iliac region. Colliquative diarrhoea thereafter set in, and on the morning of the 19th the little sufferer had rest.

Section six hours after death, assisted by Dr White. Body much emaciated. No oedema of legs or elsewhere. Nodulated gland in *left* axilla. Abdomen greatly distended, glazed, superficial veins well marked. On opening it a small amount of fluid escaped, and in its wall, in *right* iliac region, was a nodule size of a filbert. Intestines were greatly distended, and bound down towards mesentery, which was much thickened. A portion of the small intestine in *left* iliac region, about 3 inches in length, was found to be the seat of a hard cancerous mass, involving the whole lumen of the bowel, and extending to the mesentery; its free surface showed some small blood extravasations. *Liver* was enlarged; its upper surface had a fatty aspect, and on its under were three cancer masses, those in *right* and quadrate lobes small, that in *left* size of large walnut. In *right* flank (where hard swelling noticed in life) there was a cancer mass size of orange, passing down to but not connected with kidney. *Right kidney* was half as large again as *left*, which was larger than natural, with cancerous nodules in both. At lower end of sternum, on its under surface, was a nodule. Spleen, heart, and pericardium were natural. *Lungs*—*right* pressed towards front by a large circumscribed cavity behind, extending up to fourth rib, and down nearly to base. It contained some drumly serum. *Left* healthy, with exception of some pleuritic effusion at base. Head not examined. Microscopic sections of the masses, kindly made for me by Dr Stalker, pathologist to the Infirmary, showed all the characters of scirrhus.

Remarks.—In reviewing this case, we are struck with its rapid

progress to a fatal issue from the onset of symptoms. From the mother's first noticing that the child was somewhat out of his usual little more than a month elapsed, and just three weeks from my seeing him. As to the actual duration of the disease we are in the dark, and equally so as to the organ primarily affected. The large mass in right flank, free from liver and kidney, and the greatly thickened condition of the mesentery, lead me to think that the starting point was either in this mass or in the bowel—though by far the larger number of recorded cases of cancer in the child have had their seat primarily in the kidney. Thus of 29 cases in children noted by Hirschsprung 15 were of kidney. But as showing the extreme rarity even of kidney cancer, Steiner found only 4 cases thereof among 100,000 children treated in the Children's Hospital at Prague. It seems that cases of cancer of kidney occur mainly in two periods of life, viz., early childhood up to age of about five years, and old age. Thus of 67 cases collected by Dr Wm. Roberts, in his work on "Urinary and Renal Diseases," 25 occurred in children under ten years of age, and of these 22 were under five years. And again, of 52 cases collected by Ebstein, 20 were in children under ten, 16 of whom were under five. Dr Braidwood, in *Liverpool Medical and Surgical Reports*, describes four cases which came under his notice in children under two years of age—in one instance the disease having been first observed when the child was four months old. Sometimes cancerous kidneys grow to an enormous size; one in a child of four has been seen that weighed over 16 lbs. In fact it is in childhood, that not only comparatively, but almost absolutely the largest cancers of the kidney are found. Next to kidney, the liver seems to be the organ in the child most liable to be primarily affected, but even in it extremely seldom. Frerichs, with his large experience, never encountered a case in a subject under twenty years of age. Dr Farre records three cases in children of two and a half, two and seven months, and of three months respectively, and Dr West one in an infant of eight months. Cancer of stomach is a decidedly rare event in the earlier periods of life, but a case of congenital scirrhus of stomach is on record, though ordinarily when occurring in this organ during childhood it tends to assume the medullary form. Twenty was the earliest age in which cancer of rectum was met with by Dr Curling; and the same limit held good of 22 cases of peritoneal cancer noted by Dr Bristowe in St Thomas's Hospital. Of 72 cases of cancer of lungs collected by Ebermann, only one was below nine years.

In conclusion, I have to state that I am indebted to Reynold's *System of Medicine* and Ziemssen's *Cyclopædia* for the statistics as to the occurrence of cancer in the child here brought forward.

VIII.—HISTORY OF AN EPIDEMIC.

By W. B. GARVIN, L.R.C.P. & S. Ed.

THE epidemic I am about to briefly describe raged for nearly three months in Ansty, a village but three miles from Leicester. The history of it is based upon over more than one hundred cases personally attended.

Ansty from a sanitary point of view is very defective—every year about early summer, some sort of fever breaks out. Last year it was scarlet fever. This year it was, I don't know what it was. The villagers are almost all of them engaged in the shoe-trade. The houses are for a country village very close together. I know it for a fact that fourteen families use but one water-closet, which is not emptied for months.

The fever showed itself in four distinct types. They are described in their order of appearance. The fever attacked mostly young folk. The proportion of adults taken with it was about twenty per cent.

Type I.—Onset very rapid. Symptoms—Intense headache, sickness. Temp. 103°. Death in 24 hours from meningitis. There were two cases of this type.

Type II.—Onset very rapid. Symptoms—Intense headache, sickness and bilious vomiting, epistaxis, earache. Temp. 102°. Average duration five days. Recovery by crisis and attended with labial herpes and pseudo-rheumatic pains.

Type III.—Onset very rapid. Symptoms—Intense headache, sickness and bilious vomiting, sore throat, erysipelatous blush on face. Temp. 102°. Average duration seven days. Recovery by crisis, attended with labial herpes and pseudo-rheumatic pains.

Type IV.—Onset very rapid. Symptoms—Vague pains all over the body, no swollen joints, urine scalding. Temp. normal. Average duration six days. Treatment—A purge against the constipation which was present in all the cases. Bromide of potassium to relieve the headache and give sleep. Bismuth and hydrocyanic acid for sickness. Tinct. opii. for the pseudo-rheumatic pains. The other symptoms were treated in the usual way. The violent earache was cured by the glycerine of carbolic acid dropped into the ear affected. The question is, What is this fever? One doctor told me he would be inclined to call it febricula with intense headache. It could not be scarlet fever. There were no spots; only last year half the village had been attacked with that fever. It could not be typhoid or typhus for obvious reasons. It was suggested to me that probably it was epidemic pneumonia, from the fact of there being labial herpes. There were no signs of lung mischief, and cases of pneumonia don't run about on the third day. My own impression at the first, and to a certain extent still, was that the fever was epidemic cerebro-spinal meningitis. One case began with convulsions; there never was any pain in the region of

the spine. All the pain was in the head. The liver was enlarged slightly in all the cases. It is clear to me that this epidemic was caused by the filth of Ansty. Of filth as the origin I am positively convinced. In the absence of a diagnosis I shall content myself by calling it sewage fever.

IX.—MEDICAL ESSAYS AND REVIEWS.—No. II.

By JOHN BOYD, M.D., Slamannan.

AMONG the Portuguese publications that have come to hand prominent mention is merited by a small collection of papers headed "Questoes Hygienicas, Mephitismo Animal, Esgotos da Rio de Janeiro e sua influencia sobre a Saude Publica"—the Drains of Rio Janeiro and their influence on Public Health; concluding with "Alguns Conselhos Hygienicas ao Povo"—Some Hygienic Counsels to the People—by Dr Joao Pires Farinha, who is Physician of Prisons in the Brazilian capital. He begins with a review of the opinions of various authors, mostly French, on the influence of putrefying animal matters, dejecta, etc., on the general health, quoting some as to these being innocuous, others highly prejudicial, and sums them up by remarking that these emanations, if usually harmless, in certain circumstances may become most deleterious by general differences, varying degrees of condensation, condition of the locality, atmospheric pressure, and the continuance of residence of the individual in these localities. Some amusing notices from Fleury are related as to the shifts for the obtaining of subjects effected by the celebrated anatomist Antoine Dubois, of Paris, of whom his son, Paul Dubois, narrates that he attained to 83 years, and was in perfect health all the time, notwithstanding his apartments, even his sleeping chamber, were stuffed with anatomical macerations and preparations, which remind us of the resurrectionist traditions of the ante Burke and Hare epoch. A historical sketch of the conditions of Rio Janeiro as regards drainage is prefixed to the interesting account of the existing sewerage of that city. This was first constituted by a series of open ditches commenced before the second French invasion in 1711, and then with defensive weirs enlarged into a canal, which received the storm-water and all the household refuse and ordure of ten streets from Rua de Cano to Prainha, with that of the Seminario de S. José and the prison de Aljube to boot. This waterway was so badly levelled that in high tides the water was refluxed to the Rua da Alfandega, while the torrent-rains (*chuvas torrenciæ*) caused the adjacent streets to be flooded and the sewage matters deposited in all directions. These made the neighbourhood so unhealthy that few escaped the incessant epidemics of putrid fevers. Moreover, myriads of mosquitoes originating in these ditches added largely to the nuisance. They were partially covered with paving-stones, but this measure seemed to augment the virulence of the mephitic

emanation. In proportion as the city increased additional ditches were excavated, which only served to render the hygienic status worse, and not to drain off the tropical storm-waters; no scientific plan was adapted to purify the difficult topography of that capital, surrounded by gigantic mountains and containing numerous rocky masses within its bounds. This condition continued till the creation of the Companhia City Improvements in 1857,—the gloomy spectacle was daily exhibited of long strings of negro slaves or freedmen, each loaded with a barrel charged with faecal matter, proceeding in procession from the parishes near the port, depositing their burdens in adjacent fields, assailing the noses of every passenger embarking or disembarking, and putting everybody passing that way in mortal terror of coming in contact with them, or, as termed in Rio, of being “attacked by a tiger!”

The Companhia City Improvements was empowered to organize a system of mixed drainage, *i.e.*, separate drains for rain water and for sewage, in the fashion of the plans adopted in Leicester and other English cities. Practically those drains are divided into five large districts, acting independently. The main drains, or *galerias*, are brick built, oval, and from 1·10 metres to 2 metres high and 1½ metres in width. The solid contents are raised at fixed distances, and disinfected with sulphate of alumina, lime, and charcoal, and these dried and converted into *proudrette*, beneficially utilized in agriculture. The water-pipes within the city do not appear, from inadequate levels, to carry off their contents duly, hence from stagnation or intense heat they are said to be insalubrious rather than otherwise,—so much so, that lively debates are carried on as to whether the city improvements are healthy or otherwise. Statistics of the mortality of Rio de Janeiro from 1853, before the institution of this drainage system, to 1879, appear to give weight to the accusation, thus:—

From 1853 to 1856	the average amount of mortality was		8,696
“ 1857 to 1860	“	“	9,766
“ 1861 to 1864	“	“	8,525
“ 1865 to 1868	“	“	8,944
“ 1869 to 1872	“	“	9,696
“ 1873 to 1876	“	“	12,758
“ 1877 to 1879	“	“	11,541

In other cities the mortality has been observed to notably diminish after the institution of similar public undertakings, but the reasons for the non-attainment of such favourable results in the Brazilian capital appear to consist in the rapid increase of population; the marshes still remaining within the city bounds; the mud allowed to lie and dry in the streets; neglected water-tanks and ponds; the *cortiço*s or hives—masses of over-crowded habitations, where families and individuals swarm with poor immigrants, Italians, etc., where every hygienic rule is outraged; to the hospitals scattered all over the city; to the slave-markets, where

the unhappy negroes dwell in even worse conditions than the Italians in the *cortiços*; to the continual excavations of the water and gas companies; to the great cutting down and consumption of trees for building purposes; and finally, to the abrupt mutations of temperature incident on weather changes. The importation of yellow fever from infected ports is another important factor in this dismal list of morbid causes. With reference to the last named, Dr Farinha advises during its prevalence the use of carbolic acid and coal tar,—the former to be sprinkled through the streets, fosses, drains, etc., the latter to be largely burned in the infected districts. For his ably written and most instructive essay, the author has laid us under indubitable obligations.

From the Brazilian capital we have lately received what promises to be an important serial in professional literature—*Revista dos Cursos Práticos e Theoreticos da Faculdade de Medicina do Rio de Janeiro*—Review of Practical and Theoretical Courses, by the Medical Faculty of Rio de Janeiro—under the charge of five professors in that university. The first paper, by the Professor of Clinical Surgery, V. Saboia, is a study of pseudo-arthritis subsequent on fractures of the limbs, which is sufficiently erudite, sensible, and instructive, illustrated by the relation of five cases recently treated in the Hospital da Misericórdia. The author appears to consider that in cases of false joint the surgeon is generally to blame,—an axiom to which considerable exception may be taken.

Rather an amusing relation is given by Professor Souza Lima of a case of post-mortem examination of a newly-born infant at term and fully developed, which was judged to be an infanticide, from presenting two ecchymotic contusions on the temporal and infra-maxillary regions, a reddish glow on the sternum, and a perforation on the upper palatine vault leading to the cranial interior. The lungs floated freely in water, and the evidence of the child being born alive and subsequently killed appeared to be complete. To Dr Souza Lima's surprise two pupils presented themselves, and stated that the baby was the product of a difficult parturition whereat they attended—a breech presentation, wherein they had to perform partial craniotomy to complete the delivery. The prolonged passage through the canal and the special presentation appears to have admitted air into the foetal thorax; which case causes some doubt to be thrown on the test of pulmonary docimasia as conclusive of distinct respiration and live birth. The same author gives a notice of cadaveric examination of a negro found killed by a gun charged with shot No. 8, passing obliquely upwards through the thorax; and from the precise and careful way in which the report was drawn up it reminded us of some of Dr Littlejohn's work, only he might not have referred so largely to the views of Ollivier, Briänd, Chaudé, and Lucheze of the Medical School of Angers.

A lecture on Blepharoplastia in the Clinica Ophthalmologica is

given by Prof. de Gouvea, wherein, after a definition of the subject, all the various processes, from that of Dieffenbach, modified by Szymanowsky, to that of Wolfe of Glasgow, are enumerated and illustrated by well-executed woodcuts, which, with the lithographic plates at the end of the number, show that the Rio artists can compete with those of any metropolis. A *résumé* of the latest discoveries as relating to the essential element of cholera from Pasteur, Roch, Snow, Macnamara, and others, is given by Prof. C. de Freitas, which is highly readable and creditable, though not perhaps advancing much that is new. From the laboratory of Anatomia Pathologica Dr J. M. Rezende gives an account of a discovery of a liver with a single lobe, triangular in form, found in the cadaver of a negro, and numerous German and French anatomists are quoted displaying the rarity of this anomaly. The lecture of Prof. Erico Coelho on the Limits of Clinical Experimentation; the paper of Lima e Castro on the Sub-periosteal Resection of the Femur; the concluding observations by Prof. Torres Thomeim on a very rare case of Stenosis of the Tricuspid Orifice diagnosed during life and verified by the autopsy, made me feel gratified by the renewed acquaintance with the Portuguese that enabled me to enjoy the perusal of them.

In the 5th Sept. No. of the *Weekblad van het Nederlandsch Tijdschrift voor Geneeskunde*, I see a notice of the vaccination at Rio Janeiro against yellow fever, introduced by Dr Domingo Freire, which has obtained so much confidence that the Mexican Government is endeavouring to secure the application of it at Vera Cruz. Freire's *procédé* was regulated at the expense of the Brazilian Emperor by Babourgeon, a pupil of Pasteur, and many people, especially captains of vessels, have voluntarily subjected themselves to this prophylactic. This is highly befitting in the subjects of that monarch who, when he entered a scientific reunion at Paris, caused some commotion, and the audience were calmed by the remark that there was only an additional philosopher present—"il n'y a qu'un savant de plus."

We are favoured with some numbers of the *Transactions of the Sei i Kwai*, a society for the advancement of medical science in Japan. The June number commences with a notice of the Roma-ji Kwai—the name of a society recently organized in Tokyō, with the object in view of establishing a uniform system of transliteration of Japanese in Roma-ji or Roman letters, and of gradually introducing the same as a substitute for the present system of writing with Chinese ideographs, and Japanese *kana* or alphabet. Recognising the advantages which the introduction of such a system will give to the student of medicine, as well as to the progressive practitioner, by bringing them, so to speak, in closer contact with the knowledge of which they are in search, the councils of the Sei i Kwai have decided to devote a portion of the *Transactions* of the Society to articles in Japanese written in Roma-ji. We have English reports of the meetings of the Society,

the 169th, with Dr Takaki, F.R.C.S., in the chair, when a curious case of whitlow simultaneously involving the tips of both middle fingers of both hands in Takenouchi Yome, æt. 18, which the President suggested was due to the practice of the maid servants there, when washing the whole length of the verandahs, of pushing the wash-cloth before them.

Dr Kimura exhibited a patient, Maruyama, æt. 44, from whom there had been amputated the left arm at the shoulder-joint on account of a recurring malignant growth at the axilla. Dr Takaki considered it had been sebaceous tumour at the outset, which had burst, leaving the cyst behind, and quoted Erichsen's opinion that this may lead to the formation of a fungous growth.

Dr S. Suzuki gave the results of some experiments with the *filaria sanguinis hominis* recently done by him, in which he had used various culture processes with negative results. He narrated the history and post-mortem of the patient, who had suffered from chyluria, anæmia, and thoracic pains. Filariae were found in the blood, and he died three months after from pleuritis. The autopsy showed extensive caseous and tuberculous infiltration in both lungs, in liver, and left kidney.

Next meeting is principally occupied with cases of caries of spine; and in the following number a case of caries of the supr. maxillary bone, Miss Takasu, æt. 21, was successfully treated by Dr Takaki, and discharged much improved in appearance; while Dr S. Sato, Professor of Surgery in University College, Tokyō, gives a highly readable notice of his performance of the rhinoplastic operation in Harigaya, æt. 30, and a historic account of this newly introduced operation in Japan.

W. Taylor, M.D., Ozaka, gives a paper on Japanese Kakke or Beriberi, in which he details the results of some experiments in cultivation of a micro-organism he discovered in the blood of such patients,—flasks to flasks from 8 to 12 removes; then one hare, two rabbits, one monkey, and two dogs were inoculated. The dogs gave no results, but all the others presented symptoms of kakke, and served to inoculate others of the same species. This micro-organism was found most abundant within the sheaths of the nerves, the ganglia, the blood, and the kidneys. It was also found in the urine of kakke patients, in canal water, in rice, and in the soil. From each of these sources pure cultures were developed, and inoculation made with development of kakke in the animals inoculated. Experiments were made showing that the spores are not killed in the process of cooking rice. Hence one of the most probable sources of infection is the eating of rice thus infected, especially cold, a prevalent custom among the Japanese. From the peculiar screw-like movements of this micro-organism he named it *Beri-beri Spirillum*.

The war of Sei-nan in 1877 gave rise to a paper on gunshot wounds by S. M. Suzuki, staff-surgeon, I.J.N. He states that in the battle referred to conico-cylindrical bullets were used by the

enemy at first, but spherical bullets in the end; cannon balls and shells were used by them only to a limited extent from lack of such projectiles. This eight months' war produced 11,298 cases of wounds, of whom 9290 recovered and 2008 died, or 82½ per cent. of cures, and 17½ of cases with fatal issue. The factors of the latter were erysipelas, diphtheria of wounded surfaces, gangrene, pyemia, septicæmia, and tetanus.

In this campaign most of the amputations were performed secondarily, and with better results than in cases where primary amputation was done. Dr Ishiguro, surgeon-major of the army, thinks that unless circumstances should admit of the patients being kept at rest for some days, primary amputation should not be performed on the field of battle.

The review department in the publication is in Japanese printed in Roman characters, and the euphonious admixture of vowels and consonants, and the vocables frequently ending in *u*, give the impression at first glance of a resemblance to the physiognomy of the Roumanian medical journals we received from Bucharest.

The larger portion of this organ of the profession in the Mikado's dominions is printed in Chinese characters, looking like transcripts from the sides of a tea chest. With these, for obvious reasons, the readers of the *Edinburgh Medical Journal* will excuse me from intermeddling.

Part Second.

REVIEWS.

The Insane in the United States and Canada. By D. HACK TUKE, M.D., LL.D., etc. London: H. K. Lewis: 1885.

IN the autumn of last year Dr Hack Tuke, in company with Dr Baker of the York Retreat, paid a visit to the United States and Canada. During the course of his travels he naturally gave attention to the condition of the insane, fell much into the company of physicians who have to do with insanity, and paid longer or shorter visits to 40 asylums. The results of his observations are stated in the volume under review, an octavo of 270 pages. The first chapter, on "Early Lunacy Practice in America," is principally taken up with an account of Benjamin Rush, "the American Fothergill," a learned and sagacious physician whose ideas on the subject of insanity were vitiated by the errors of the times. The second chapter is also in part historical, dealing with "The Provision for the Insane in the United States from 1752 to 1876." Chapter III. treats of "The Present Condition of the Insane in the States," and in Chapter IV. we have short descriptions of the principal asylums visited.

The book is indispensable to those who wish to know the present condition of the asylums in the States, and the prevalent views on the treatment of the insane. It is interesting as describing contemporary events, and will become valuable as a book of reference. Dr Hack Tuke's previous works have made him known and appreciated; and in the present volume we meet the same pleasing style, the same wide knowledge, and the same catholic tone. Not the least pleasing features are the stray remarks and sagacious comments which here and there take their starts from something in his narrative.

The fifth Chapter "On the Condition of the Insane in Canada," while doing justice to the well conducted asylums in the Upper Provinces, such as those of Toronto under Dr Daniel Clark, and of London, Ontario, under Dr Bucke, contains some severe criticisms upon several of the asylums in Lower Canada. Like his friend Dr Bucknill, whose notes on "Asylums for the Insane in America" were reviewed about nine years ago in this Journal, Dr Tuke found the patients much neglected, kept under needless restraints, and stinted by penurious diet.

Dr Tuke's work, written in a tone appreciative of the good which he found, and ready to make proper allowance for deficiencies, has been already well received in the United States. In Canada his criticisms aroused immediate attention, and we have means of knowing that though it aroused the hostility of those in charge of the asylums inculcated, it has been well received by the public, and will in the end help in effecting much needed reforms.

Seven appendices, giving statistics on the number of insane persons and idiots in and out of asylums, the amount of restraint put in use, the cost of construction, and of annual board, add to the value of the book as a work of reference.

The labour of collecting the information for this work must have been very great, and the facts would have lost much of their worth without the correct and matured judgment of the author.

The Field and Limitation of the Operative Surgery of the Human Brain. By JOHN B. ROBERTS, A.M., M.D., Professor of Anatomy and Surgery in the Philadelphia Polyclinic, Surgeon to St Mary's Hospital. Pp. 78. Philadelphia: P. Blakiston, Son, & Co.: 1885.

THIS able and suggestive essay formed one of the papers read at the meeting of the American Surgical Association in 1885. It is well worthy of publication in a separate form, although its value is enhanced when it is read in the Transactions of that Association, along with the criticisms and remarks of the members who spoke on it. The subject is treated of in three chapters—I. On the Principles of Cerebral Surgery; II. On Cerebral Localization;

III. On Operative Treatment of Cerebral Lesions; in each of these again, the matter is divided into definite heads, which are thus separately discussed. The whole subject is thus clearly marked out, and the study and criticism of the paper rendered proportionately simple.

The views of the author are more advanced than those of most modern surgeons. He considers that trephining is by no means a serious operation under proper precautions (including antiseptics) and that it is to be performed "as an exploratory measure, nearly as often as it is demanded for therapeutic reasons."

After defining and explaining his nine principles of cerebral surgery, he discusses the subject of cerebral localization in a like way. We have presented to us a *résumé* of what is known about cerebral localization, and of the methods of finding out the relation of the chief sulci and convolutions to the skull in the living person. Among these, however, we miss the one published by Mr A. W. Hare in the *Journal of Anatomy and Physiology*, vol. xviii, p. 174. This chapter closes with four tables,—(1.) On Indications for Trephining; (2.) On Contra-Indications to Trephining; (3.) On the Points to be Selected for Operations in Particular Cases; (4.) On Uncertain Localization. Dr Roberts has erred on the side of defining too minutely in these tables, but they may be useful if considered from a general standpoint.

The third chapter, on Operative Treatment of Cerebral Lesions, enters fully into the consideration of six classes of cases where surgical interference might be called for, *i.e.*, cranial fractures, intra-cranial hæmorrhage, intra-cranial suppuration, epilepsy following cranial injury, insanity following cranial injury and cerebral tumour. Each of these heads is dealt with in turn, and much valuable information is brought before us. Throughout the essay accurate definition and clearness are the prevailing features. The literature of the subject has been well studied, and is frequently referred to in order to exemplify the many points considered. The author is to be congratulated on this important contribution to the study of cerebral surgery.

Practical Surgery: including Surgical Dressings, Bandages, Fractures, Dislocations, Ligature of Arteries, Amputations, and Excisions of Bones and Joints. By J. EWING MEARS, M.D., Lecturer on Practical Surgery and Demonstrator of Surgery in Jefferson Medical College, etc.; with 490 Illustrations. Second Edition. Philadelphia: P. Blakiston, Son, & Co.: 1885.

IF the word "practical," as applied to surgery, is to be taken as meaning operative or at least manual, we cannot hold that the title of the present work has been a fortunate one. There seem to be mainly two conditions which a book with a title such as the above might be expected to fulfil. One of these is, that it should treat

of at least the majority of practical subjects in surgery, and the other is that it should treat of them in a practical way. Neither of these conditions seem to us to be adequately fulfilled by Dr Mears' work. Of operative procedures none are discussed except those which come under the head of ligature of arteries, amputations, or excisions. Why the line should have been drawn at these we find it difficult to explain. It is not enough, however, merely to discuss practical subjects. If they are to be treated in a practical way, they should be handled with something of that vivid freshness which can only come of observation at first hand. To ensure this either the exact words of authors should be given, or the compiler should be able to impress upon the reader his personal knowledge and individual conviction. The exact words of authors are not quoted, while, at the same time, the opinions referred to by Dr Mears do not seem to have been always well digested by him. This is perhaps most noticeable in the all-important subject of wound dressing. In one place we have a faithful account of antiseptic methods and dressings, and their importance is strongly urged. At other places, scattered throughout the book, remarks are made which make it appear as if Dr Mears had neither thoroughly grasped the principles of antiseptic surgery nor realized their importance. For instance, in treating of compound fractures of the skull, he says, "The local dressings should consist of compresses wrung out of cold water, until the cranial opening is closed." . . . "Antiseptic dressings may be used with great advantage in certain cases," p. 155. Again, speaking of wounds on page 61, he says, "The cleansing of wound surfaces, which are in contact, may be effected by injecting tepid water or warm antiseptic solutions through the drainage-tubes which have been introduced."

The chapters on Fractures and Dislocations are unsatisfactory *résumés* of what can be found in any good text-book on surgery. The same remark also applies to the parts of the book which treat of Ligature of Arteries, Amputations, and Excisions. In none of these latter sections are the probable reasons for the operations given, nor any indications of their results in other hands by reference to statistics.

The illustrations have been collected from the various sources mentioned in the preface. Their original clearness and artistic value have suffered much in the process of their transplantation. Altogether, while *Practical Surgery* contains much useful information, its deficiencies are considerable.

The Treatment of Urethral Stricture by combining Internal and External Urethrotomy. By REGINALD HARRISON, Esq., F.R.C.S. London: John Bale & Sons: 1885. Reprint from *Brit. Med. Journal*.

THE title of Mr Harrison's pamphlet sounds as if he were pro-

posing a work of supererogation in suggesting both the internal and external urethrotomy in the treatment of stricture. On reading his paper, however, one sees that the proposal has sound common sense in it, and is based on sound surgical principles. The idea is simply this—to tap the urine behind the stricture, and so to give complete rest to the raw surface produced by operating on the stricture. Mr Harrison considers that there is some risk in permitting urine to flow over the raw surface produced by an internal urethrotomy, and to that he ascribes the rigors and other troubles which he has seen follow the operation. In proof of this, he says that in twelve cases in which he has performed the double operation no unpleasant symptom occurred. His procedure is as follows:—He first employs a urethrotome (Watson's or Maisonneuve's) to divide the stricture, then passes a grooved staff and opens the membranous urethra on it. A tube is then passed into the bladder by the perineal wound, through which the urine drains away for some days. The anterior portion of the urethra is syringed out with an antiseptic solution twice or thrice daily. The pamphlet is well worthy of careful perusal by all practical surgeons.

Litholapaxy. One Hundred and Eleven Cases. By P. J. FREYER, M.A., M.D., Bengal Medical Service. Calcutta: Thacker, Spink, & Co.: 1885.

DR FREYER'S pamphlet is a valuable addition to the literature of a subject which does not lack contributors. Among his 111 cases there were 4 deaths—not a large proportion; and examination of these fatal cases shows that they were mostly hopeless from the first.

Dr Freyer writes boldly, and gives his own experiences without slavishly following authorities. What he says is of all the more value. He does not spare Sir Henry Thompson for first condemning Bigelow's operation, and then trying to make out that he had found it out for himself.

Dr Freyer considers litholapaxy as *the* operation for vesical calculus in India, and for adults. He gives one or two valuable practical hints—such as that the use of the aspirator and evacuating catheter may assist the diagnosis of small calculi, for when the injecting fluid is made to rush out and in of the bladder, the calculus may be brought against the catheter, and a click be felt, or the stone may even, if very small, be brought away by the return stream along the catheter. Surgeons who have not already read the paper in the *Lancet* will find it interesting and suggestive.

A Physician's Pharmacoposia, containing Formulæ of Unofficial Preparations. By J. BAILY, Pharmacist, Margate. London: J. & A. Churchill: 1885.

WE are told on the title page that it is written "with the object

of securing uniformity in dispensing, and to discourage secret nostrums." It appears to us that the book is little else than a vaunting before the profession of the "*nostrums*" of Mr Baily. His proposal that all tinctures should be of the strength of "1 in 10," is one which can never be accepted by the profession. The book is elegantly bound, but this is its only recommendation.

Veterinary Pharmacology and Therapeutics. By JAMES BRODIE GRESSWELL, M.R.C.V.S., etc. London: H. K. Lewis: 1885.

THIS is an excellent epitome of veterinary medicine and therapeutics, and is sure to be extensively used by students and practitioners.

Lewis's Pocket Medical Vocabulary. London: H. K. Lewis: 1886.

THE intention of this little work is a good one. A Medical Vocabulary which may find a home in the waistcoat pocket, without displacing the odd change and other multifarious articles which cannot be stowed anywhere else, will prove a desideratum to the over-lectured student or harassed practitioner, whose reversionary interest in the article time is gradually being reduced to an infinitesimal quantity, and who might, consequently, prize this tiny aspirant to etymological light and leading. While by no means a *multum in parvo*, there is an amount of unutilized space which might have been available for carrying out with more completeness the apparent plan of the work. The etymon of some words has been supplied, but this has not been consistently carried through. For example, the derivation of "Abulia" and "Acarpus" is given, while that of "Acardia," for some inexplicable reason, is not quoted. It is in its references to the Greek, however, where the special weakness of the work consists. Here the errors and misprints seem

"Thick as autumnal leaves that strew the brooks
In Vallombrosa."

The accentuation is specially defective, which is worse than awkward when asper and lenis get hopelessly misplaced, as in the following, for example,—"*ελμινς*, a worm," "*ανεμος*, the wind," "*ερνος*, sprout;" while the inquirer after knowledge is informed that "Glottis" is derived from "*γλωσσις*" (*sic*). With the impartiality which characterizes the impersonal reviewer, we feel bound to condemn the work as alike unworthy the confidence of the profession and the character of its publisher, until its Greek, at any rate, has been revised by one competent for the task.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LXV.—MEETING VI.

Wednesday, 17th February 1886.—Professor GRAINGER STEWART, *President, in the Chair.*

(Continued from page 1078.)

The President said that the reception which the meeting had accorded to Mr Lawson Tait's address showed how highly they had appreciated it. They looked forward to an interesting discussion that evening in connexion with his views, which were so far-reaching and so bold, and manifested features of a wholesome conservatism; and the plan which the Council thought best was this, that—as they had a number of friends present—they should request one or two to make remarks, and that the meeting should thereafter be thrown open to those who were willing to give them any observations. He would, therefore, call upon some gentlemen who had been suggested to him at the beginning, and then others would have an opportunity of speaking. The Council proposed to limit the time, and suggested that those who spoke first should not speak more than ten minutes each, and that those who spoke later should confine their observations to five minutes duration.

Professor Simpson, who was first called on by the President, expressed his gratification at being able to take part at that interesting meeting, where so many of the members of the Medico-Chirurgical Society, and so many of the gentlemen who would form the future members of it and of the profession, had been gathered together to hear the experiences of one of their most distinguished old students—a man of whom he might say, as a statesman said of Palmerston in his day, “we are all proud of him.” There was so much to talk about that they had not time to stay in the region of compliments; and he would, first of all, take up the largest question, to his mind, which Mr Tait had opened up to them. That question was, Whether the interference with the abdominal cavity was to be kept in the hands of a few specialists? whether they were, as a profession, of set purpose and from the beginning to say, “some men”—and they could only be a few—“shall have all the cases of laparotomy that are ever to be performed in this Great Britain of ours”—not to speak of the rest of the world? As Mr Tait had stated it, it was a very far-reaching principle, it seemed to him. What held good for the abdomen should hold good for the other regions of the

body; what held good for ovariectomy should hold good for other operations; what held good for surgery, he suspected, would have to hold good for medicine. Now, he believed that the greater proportion of these operations would fall into the hands of the men—and rightly so—who could show themselves to be most successful in the performance of them. But how were these men to begin to be successful? If a young practitioner found himself in contact with a promising case of ovariectomy, was he to say—"Oh, Dr Keith and Mr Lawson Tait never have any mortalities at all, and how do I know but that in this case the woman will die, and then her death will be on my conscience for ever? I think the patient must be taken somewhere else." Under that system there would be no opportunity for a young man to try his hand in cases where he conscientiously believed he could perform any capital operation. That would lead them back to the time of the Chamberlains, now two centuries ago. There was a family of them, who certainly had a power of delivering women in cases of difficult labour which other practitioners did not have. They had the secret of the forceps. He wanted to know if it would be the sense of the profession that they should send all their difficult midwifery cases to men who had skill in an instrument which had been unrivalled? There were some men who had success in the operation of lithotomy. According to Mr Tait's assumption, a surgeon who had a fatal case of lithotomy ought to cease to cut for the stone, and send his patients to some successful lithotomist. Then the principle must be extended further to the field of general medicine. There were some practitioners who had had success in the treatment of diabetes, kidney disease, or whatever else it might be. The general practitioner would have to say, "Am I never to treat kidney disease according to my understanding because some more advanced practitioner has had more success?" He thought Mr Tait had laid down the principle somewhat too broadly; and, further, he was to protest against it in the name of teachers of coming generations. He agreed with something Mr Tait said as to the desirability of having fuller teaching in the two departments that fell under his care—obstetrics and gynecology. But how was abdominal section to be learned? Mr Tait was not a teacher; Dr Keith was not a teacher. They could not send their graduates to all parts of the country and all parts of the world uninstructed in the technic of these abdominal sections, and it was impossible to give instruction in operative procedures of which one had no practical experience. He was free to admit that no teachers that he knew of had a success at all to compare with that of the two laparotomists he had named, because, perhaps, they had not the time to devote their energies to one single operation. The patients would mainly find their way into the hands of those men who were most successful; but he must claim for teachers the right, when they recognised they had a case they could

operate upon, to operate on it at their own hand, and not pass it away to another man, who may have had a brilliant success. It was an extremely interesting paper which they had heard that night, because Mr Tait had lived through, and had been able to set very clearly before them some of the successive phases in the progress of surgery as to sections through the abdominal walls. It certainly must be to the older members of this Society a matter of great surprise to see the boldness of surgery in the present day, because in it some of the most eager discussions were maintained for long years, in regard to even the justifiability of that operation that led the way to all the others—that of ovariectomy; and he supposed that if some of the surgeons of twenty-five years ago could have been among them now, they could hardly have believed their ears to hear that the abdomen was to be opened, not only for the removal of ovarian growths, but in cases of puerperal peritonitis or inflammatory exudations of the pelvis. And yet none of them could doubt that Mr Tait had presented a fair case for section of the abdomen, even in such cases. He was convinced Mr Tait was giving sound advice when he told them that among such patients there were chances of recovery, and that they might be able to save life that would otherwise be lost. He had himself seen such a case on the post-mortem table, and wished—that was several years ago—that one had opened the abdomen when the woman was still alive, and got rid of the accumulation of pus in the pelvic cavity and Fallopian tubes that was the result of gonorrhoeal inflammation. No doubt, in such a case as Mr Tait had told them of, the matter might have been cleared out, and the woman might have had a chance of recovery. Let him ask, in fine, in regard to this operation, what he thought were the important lessons to be learned, and in part indicated in Mr Tait's paper, as to what were the chief conditions of success? He thought that was what they ought to lend their minds to try and discover. He quite freely granted that the greatest stress should be laid on constant experience in handling the peritoneal viscera through the abdominal walls. It had been proved in the experience of operators, even if they had gone through their 500 cases, that the oftener one operated the more safely he operated. But he thought Mr Tait and the ovariectomists should try to lay down clearly before their own minds and of those who followed them in this operation, What were the elements of success in individual cases? Mr Tait mentioned the short incision. There was no doubt he was right. The longer the incision in the abdominal wall the greater was the danger. The second point was the intraperitoneal treatment of the pedicle. There again he was in accord with Mr Tait. The dropping of the ovarian or uterine pedicle into the cavity, so that the wound might be thoroughly closed, was a great element of success. Yet again, Mr Tait spoke of the opportunity of early operation; and he

agreed that too great stress could not be laid on the importance of having recourse to an operation early in any case where they deemed it in the least wise to lay open an abdominal cavity. Another subject he thought Mr Tait might have touched on, namely, the element of rapidity in the operation. The cases which one had most fear of were those where, from whatever cause, the operation had lasted a longer time than usual; where, from the time lost in separating adhesions and securing the bleeding points, the patient had been an undue length of time on the operating table. Every minute on the table, to his mind, increased the chances of danger, and therefore it became important for the more experienced to tell them what expedients had commended themselves to their judgment, to enable them to get quickly through the operation, so that they might try to follow them, at however long an interval, and adopt all the measures that they found most useful for carrying through, as speedily as possible, an operation in which speed meant safety.

Professor Annandale, who was next called on, said he should wish, for his own sake, to thank Mr Lawson Tait for his most interesting address. He should further wish to congratulate him on his success, and to wish him every further success in the important department of surgery which he was connected with. He had had little experience in connexion with special uterine or ovarian surgery, because following that principle of his old master Syme which Mr Tait had referred to, and knowing there was a Keith in Edinburgh, he had always sent his cases to Dr Keith, because he felt that the interests of the patient were better in Dr Keith's hands than in his own. He would like, however, to make a very few remarks on abdominal section in general surgery, more particularly when there was suppuration in the abdominal and pelvic cavities. Now, it was more than twenty years ago since he learned that aspiration and the exploring needle were not to be depended on for the diagnosis or removal of pus from the abdominal or pelvic cavity. He well remembered the case that taught him that lesson. He was asked to go to Fife to see a farmer who was supposed to be suffering from malignant disease in the abdomen. He found him emaciated with hectic fever. On examining the abdomen, there was some dulness in the iliac region, but not very well marked. His diagnosis was that there was some suppuration in the pelvis. He introduced a long aspirator needle and no pus came, rather to the satisfaction of the medical man, who thought he was on the wrong line altogether. He went into the garden and had a walk, and, after consideration, said he was sure there was pus there. "I do not think so," was the reply. Well, he said, if the farmer would allow it, he would explore his abdomen. The farmer gave permission, and he cut into the abdomen. He might mention that he made the incision in the left lumbar region, and having scratched through some adhesions

of the peritoneum, he opened into an abscess cavity containing a renal calculus, and the patient made a good recovery. Ever since that time he had carried out that principle, and he never hesitated in any case where he thought there was sup-puration of the abdomen or pelvis to make a free incision and investigate, and generally he had been rewarded with success. He thought it only fair to the Edinburgh School, and he hoped they would excuse him if he seemed rather egotistical, for he was not so personally, but with regard to their School, in order to prove that it had not been behind on this important subject, to refer to a paper which he published in the *Edinburgh Medical Journal* in January 1875. This paper was the result of his own experience of the value of abdominal exploration, and he would give the following quotation from it:—"In abscesses or suppurations occurring in the abdominal or pelvic cavities, or in some of the important surgical regions, the presence or absence of pus cannot always be detected by the exploring needle, and I have on several occasions discovered a collection of pus by an incision, having previously failed to obtain the information by the use of the former means." In fact, he thought they would acknowledge that Mr Tait and himself had been at one for many years in regard to the importance of exploratory incisions, and especially in connexion with abdominal disease. He had never been sorry that he did open into the abdomen in connexion with such cases; but he had been sorry on one or two occasions in which he did not open into it. He had met one or two cases of inflammation in which it was difficult to tell whether the disease was of an inflammatory or cancerous character, and one case two years ago made a great impression on his mind. It had all the symptoms of cancerous disease. He met in consultation several distinguished gentlemen, and they were all of that opinion. He had a very slight doubt in his own mind, but still he was influenced, naturally, by the opinion of the others, and agreed that it was a case of malignant disease, and had to break this sad news to the patient's wife. But it turned out that it was not malignant disease, because the patient recovered. It had been a case of inflammation, and he was now sorry that he did not make a small incision, because that would have decided the matter, and, instead of telling the patient's wife that he would never get better, he would have been able to give a more favourable prognosis. Another important subject was that of section in cases of intestinal obstruction. In regard to these cases he had always made it a practice, in acute cases of intestinal obstruction, that if in the course of twenty-four hours the obstruction was not relieved, he opened into the abdomen, and in many cases he had found conditions that were removed very simply—sometimes a band of adhesions, a twist, or a slight invagination; but, in other cases, he had found conditions that

could not be relieved. But he had never been sorry he did open the abdomen, because the conditions could not have been cured by other means, and were not aggravated by the operation. In regard to renal surgery, they knew this was a subject in which he was much interested, because he had the credit of being one of the first to cut down upon the kidney in order to test whether or not a calculus was present in it. In the first case he did not find a calculus; but the extraordinary thing was that though the patient's symptoms were those of calculus, and he saw the patient with Dr George Balfour, who was also strongly of opinion that there was a calculus, when he recovered from the operation the symptoms were quite relieved, and he thought it always wise to make an incision in cases of suspected calculus when the symptoms were unrelieved by ordinary treatment. Referring to the question of abdominal section in cases of injuries in the abdomen, there was no doubt but that surgeons generally had been too chary of cutting into the abdomen in cases of injury. Last year he had a case of a boy with a pistol shot wound, where there were seven or eight wounds in the intestines. He opened the abdomen, stitched up the wounds, and ligatured a large mesenteric vein. The patient did not recover, but he was sure he put him in a better condition to recover than if he had left him alone. In cases of rupture of the intestine or bladder there could not be a doubt that the best mode was to make an incision, suture the wounded viscus, and carefully sponge out all faecal or other matters that were likely to cause inflammation.

The President.—In ulcer of the stomach do you advocate that?

Professor Annandale.—Certainly. Before sitting down, the Professor said he wanted to tell an interesting historical incident in connexion with a case Mr Tait had referred to, that as to the condition of suppurating peritonitis. He did not think Mr Tait knew that twenty-five years ago he had operated for suppurating peritonitis; and it was only fair to say that he did not do it on his own suggestion. But it so happened that he was house-surgeon in 1861, and the late Professor Hughes Bennett sent for him one day, and said,—“I have a patient here with suppurating peritonitis—peritonitis caused by a fall from a house—and what I want you to do is to take this man and put him in a warm bath, and tap his abdomen with a large trocar and canula, and, having drawn off all the pus, to take a syringe, when he is in the bath, and inject warm water into his abdomen, until it comes out pure and clear.” He did so. The patient did not recover, but he believed now, that if he had opened the abdomen, washed it out, and drained it, possibly the result would have been more satisfactory.

Dr Maclaren, Carlisle, on the invitation of the President, said he should not have troubled the meeting with any remarks, were it not that he thought he could present some facts to them from a different point of view than that of those who were pure

surgeons, or were specially engaged in abdominal surgery. He occupied the position of surgeon in a hospital holding 100 patients, about 30 of whom were patients under his care. For many years he had treated all surgical cases that had come in on his admission days, whether they were abdominal or belonging to any other department of surgery, and he thought it might interest them if he compared the number of abdominal operations with a few of the other grave surgical operations. He had opened the abdomen, to his knowledge—he made that qualification, because in certain pelvic cases he could not be certain whether he had opened the peritoneum or not—twenty-six times: eight of these were for cysts, one for pyo-salpinx—practically these were identical operations; one died. Three operations were done for non-cystic ovaries; three were exploratory; four were for intra-peritoneal abscess; five were for intestinal obstruction; one was for removal of the uterus; and one for a sarcomatous tumour involving the peritoneum. Now, of operations which he would compare with these, he had done amputation at the hip-joint six times; removal of one-half or the whole tongue, four times; of tracheotomy he had not a correct record, but his cases amounted to about thirty (hospital and private). He had opened the bladder three times; of amputations of the thigh, again, he had not quite a correct record, but they were between thirty and forty; for hernia he had operated sixteen times; he had never tied a large vessel in its continuity. He had an object in presenting these figures to them—that was to urge the need for better education in abdominal surgery of all those who were entering medicine. It was his experience, as he had already, on a former occasion, stated to the Society, that when a house-surgeon had to help him with an abdominal section generally the first question he asked was—"Can you give me some book that will tell me something about the operation in view?" As a rule, newly passed men knew nothing about it; it did not matter what school they came from. Looking into the reasons for this, he found that his friend Mr Tait was not a professor of abdominal surgery, but a professor of anatomy to a Society of Artists. Dr Keith, he did not find, was a teacher at all. He was not aware that any of the great London specialists in abdominal surgery taught it. Sir Spencer Wells was formerly a professor of surgery and pathology in the College of Surgeons in London, but it was not in his knowledge that this meant any teaching in abdominal surgery. Therefore, that section of surgery which occupied so prominent a place in the ordinary work of a surgeon was imperfectly taught. In regard to the question of specialism, it seemed to him that it was rather a waste of intellect to discuss much whether these operations should be specialties or not. The inexorable logic of events would decide the matter. He would, however, ask them to remember that operations which in his days

of studentship were only done by masters of surgery, and where he could remember the theatre in the Old Infirmary crowded with students and practitioners, crowded like hiving bees—he had seen them hanging on to the bars which supported the gallery—were now done in provincial hospitals, without any spectators except such of the staff as might drop in in the ordinary work of the hospital. Let specialists look at the matter from the individual point of view. They must remember that this island, and, indeed, the world in general, is fairly well peopled, yet there cannot be specialists everywhere. He himself was not within a hundred miles of one, and if he declined to perform an operation it involved separation from friends and the expense of a distant journey. Moreover, it was only fair to state to them that he knew of two cases in his early days where, if he had been able to say to the patients, “I can perform it with such and such a chance of recovery,” they would have submitted to the operation. Not being able to say that, the cases went unoperated on to the grave—cases which were somewhat of a discredit to their profession. They must give honour to all those who have made abdominal operations a great success. They might envy them; and he thought they might ask them to tell freely and fully why they were successful. He would sometimes like to know more than he did know. When he first operated he had only seen one ovariectomy—by Dr Graham of Liverpool—and afterwards he had to make his own experience. Now, what was experience? How far could the results of it be taught? Would specialists tell wherein they differed from ordinary surgeons? Was it simply in the education of sight and touch, or was it in any particular methods? Was it, for instance, the way of using ligatures or sutures? In short, what was the outcome of their experience? He sometimes wished specialists would tell them a little more about such things; so that every man who felt himself competent to undertake surgery, whenever an internal intestinal strangulation came in his way, should be able to cut down on it, see what was the cause, and, if possible, remedy it, and yet subject his patient to no greater risks than such are submitted to at present in the hands of the men who know best about it.

Dr John Duncan, who was next called on, said he rose not because he was hopeful of adding anything regarding abdominal surgery to the brilliant address of his friend Mr Tait, but rather with a view of seeing whether they could not deduce lessons which might be applicable to general surgery from these successes of a small knot of men—most of them, he was glad to say, hailing from this quarter. Of course it fell to the lot of the general surgeon to deal with the peritoneum and the other parts of the body equally. He must take cases that fell in his way, whether hernia or broken leg, although, like Mr Annandale, he might turn over a small class of them to Mr Keith. The first, he would almost say platitude,

that he would submit to them, was that the principles which governed operations in abdominal surgery were entirely applicable to all other surgery; that there might be details in their mode of application which differed, but that the general principles were the same. If they inquired what were those general principles of wound-treatment, they must investigate the causes which made their wounds go wrong—why, in short, did inflammation and fever arise? And the answer to that was exactly the same in abdominal surgery as it was elsewhere—that they might arise from mechanical, chemical, or organic causes. As to the mechanical causes, they might draw one lesson from abdominal surgery. He supposed that the nerve relationship of the peritoneum and the severity of the operations with which these surgeons were concerned brought it about that patients who had been submitted to ovariotomy were in larger number than others brought to the lowest ebb and yet recovered. The severity of the mechanical interference produces a large amount of shock, but shock is an evanescent condition which only proves fatal in combination with other evils. The chemical causes of inflammation, he thought, were only of practical value to them in connexion with the application of antiseptics, and the conclusion they would draw from abdominal experience was, that the less they applied antiseptics to the abdomen or to any wound the better. That was nothing new. That was a principle that Sir Joseph Lister from the very first had enunciated more strongly perhaps than any one else. But, thirdly, they had the organic, and it was in reference to this that he thought their most important lessons might be deduced. He was not going to argue the point; he took for granted the germ theory. He believed, notwithstanding what Mr Tait said—his expressions were a little strong on this point—that they would not soon abolish Listerism; that the antiseptic theory would survive most other theories. He thought they must conclude that the difference between compound fractures and simple ones, between an open wound and a closed one, was the admission of microbia, and if they admitted that, the question was how best were they to avoid the evils resulting therefrom. Well, he put aside as not german to the matter—because, probably, both abdominal and general surgeons were at one as to it—he put aside the after treatment, and he confined himself to the treatment of the injured during an operation. There were three ways in which they might endeavour to avoid the action of those microbes. One was by operating in purified air. This was ideally the most perfect plan, and Mr Lister had attempted it—he did not think quite successfully. He did not think that the spray did act in that way, but it was possible that either by antiseptic chambers, or in some other way, they might at some future time purify the air. The next mode whereby they might avoid the entrance, through the air, of living germs to the wound was by irrigation. But they

had already spoken of the evil results of the application of antiseptics to the wound. Thirdly, and this he thought the most important deduction from abdominal surgery—they might trust to the germicide properties of the tissues themselves so far as the air was concerned. He thought Mr Tait would agree with him in all probability that the air was the least important factor in the introduction of organisms, that the means whereby chiefly microbes were introduced into a wound were the fingers, the instruments, and sponges of the surgeon. In the next place, the question of quantity was of the greatest importance. The number of microbes that might get into a wound was of the greatest possible importance with reference to subsequent evils. Well, bearing these things in mind, and bearing in mind the results obtained without antiseptics in abdominal surgery, he thought that to trust to the germicide properties of the tissues, not antisepticated, not devitalised by mechanical injuries, was a reliable way of treating all wounds; and of late (his attention being drawn to it by Mr Tait's operations and others) he had been endeavouring to put in practice this principle in the case of ordinary surgical wounds. There was a patient at present in the Infirmary on whom he operated by excision on Tuesday week for disease of the elbow. He had operated before the skin had been broken by suppuration. He applied antiseptics to himself and his instruments, and not one drop of any germicide to the wound itself. He neither used spray, nor irrigation, nor anything else. He was excessively careful to arrest every drop of hæmorrhage. He took special precautions in this way with the view of having the wound absolutely dry. In order to test the case still further, and put it under the worst possible conditions that could be, he then sewed the wound completely up with cat-gut, without drainage, and covered it with flexile collodion and wool. The temperature had not risen since the operation; there had not been a drop of discharge, and the patient was now, of course, absolutely well. That, he thought, promised to be one of the many lessons which they might learn from abdominal surgery—not that Listerism was to be abolished, not that the antiseptic system was to be done away with, but that the antiseptics were to be applied to the surgeon, and not to the patient.

Dr Halliday Croom said, in common with the gentlemen who had already addressed the meeting, he had listened with very great interest and profit to the very able paper which had been read by Mr Tait, and he took this opportunity of thanking Mr Tait for his kindness to him personally, on his last visit here, in pointing out many details of his operative work which he had not had an opportunity of seeing before. It was impossible for him, of course, with his comparatively limited experience, to communicate anything to that audience of fresh interest in regard to the debate. In reference to the question of ovariectomy, when a surgeon like Mr Tait came there, and told them he had

performed 138 consecutive operations without a single death, he thought that operation ceased to be one for discussion. He could not see any room for discussion; it was not a question at all. The point was settled; it was a recognised operation; and although there might be details cropping up from time to time, the question of operation was settled once for all. Another question, to which he thought, however, Mr Tait had not directly alluded, was the removal of the ovaries with the view of arresting hæmorrhage and bringing on retrogressive changes in fibroid tumours of the uterus. He had seen Mr Tait perform the operation on a patient of his own, who had bled every week for two years. Some time in December Mr Tait had operated on this patient and removed the ovaries and tubes, and since then the patient had not lost more than a teacupful of blood altogether. He thought, as Mr Tait had omitted to speak of this operation, that he should like to draw attention to the extreme importance of removing the ovaries and tubes in these cases, because it seemed to him, from what he had seen in practice and in the hospital, that a great object would be gained for their patients if they recognised fibroid tumours earlier, and removed the ovaries when yet the tumour was small and in the pelvis. When they were allowed to grow and extend into the abdominal cavity the removal of the ovaries became a very serious and dangerous operation. He would like, further, to say a word in regard to the operation to which Mr Tait had specially attached his name—the removal of the uterine appendages where there was inflammatory conditions. Two questions he would like to ask—first, How he diagnosed those cases? Did he take his diagnosis from the following symptoms—that the patient had suffered from pelvic distress, that she had profuse menstrual discharge, that that discharge was accompanied by pain two days before the onset, and that she had pain on coitus. Did he make his diagnosis from that, or from what he felt in the pelvis, from signs? If so, then he wanted to ask Mr Tait another question, Did he do, as he had himself seen him do, when he had the pleasure of seeing him in his ward with Dr Imlach of Liverpool. He showed them three cases of what was supposed to be diseased tubes. Both Mr Imlach and Mr Tait examined the patients, with one hand only, while the other was somewhere else—in the pocket, he thought. He introduced two fingers and said, "This is a case of tubes." Now, if there was one thing they as teachers had insisted on in Edinburgh, it was that no satisfactory examination of a woman's pelvis could be made except it was bi-manual. He asked how it was possible even for Mr Tait, with his vast experience, to know what was in a woman's pelvis with one hand in the vagina and the other somewhere else. Mr Tait had revolutionized many things in surgery, and had knocked down some. Was the bi-manual examination one of these? Another point on which he should like to ask a question was, Where was the limit to these operations? Where did

Mr Tait draw the line at in removing tubes and ovaries for disease? It would be a very great advantage to those of them who were teachers if they were able to say definitely what were the cases in which this operation should be performed. For instance, he had under his charge two young women of about 27 years of age. Both had been married three or four years, and these women had both acutely inflamed and prolapsed ovaries. They suffered pain at menstrual periods; they had pain on coitus, and were sterile. He was not at all squeamish or sentimental on the subject of unsexing women, but at the same time he would hesitate before he sanctioned the removal of these women's ovaries under these circumstances. Therefore, he thought, they should have some light on the subject. Was it right, when a woman presented herself suffering from pain at her periods, and something was felt within the pelvis, was it right to make an incision for diagnostic purposes, and if necessary to remove her ovaries and tubes? Again, how long must a patient suffer before one had to interfere to take out the ovaries and tubes? Another remark occurred to him to make on the subject. He could only wish to endorse what had fallen from Professor Simpson and Dr M'Laren, namely, that Mr Tait was rather hard on those of them who were what was called obstetric physicians, when he said they should never perform these operations. Was it because the operation could not be so well done? Possibly it might be so. For his part, he thought Mr Tait and the so-called ovariologists assumed too much. He saw no reason why an ordinary physician in charge of gynecological wards—what is known as an ordinary obstetric physician with due experience—should not perform abdominal section with equal success. He could see one objection, namely, that obstetrics was an uncertain department of practice; that one might be called on a certain day to perform an abdominal operation of extreme severity, requiring all care and attention, and might have lost a night's sleep the night before, and not be in the best condition to apply his mind or hand. But the same would apply to Mr Tait if he travelled from Birmingham over night, and had to perform the operation here. Was it in the interest of the parturient woman that Mr Tait objected? Surely, with the observance of careful antiseptics, no possible harm could come to the parturient woman. At any rate, the argument told quite as much the other way, for Mr Tait might have to deal with a cancerous mass, from which it was most difficult to disinfect one's hands, and perform an ovariectomy shortly afterwards, *without any antiseptic precautions whatever*, and according to his own showing he had practically no deaths at all.

Dr Imlach, Liverpool, then said—When he was honoured with an invitation to take part in this discussion he accepted it chiefly that he might hear what Mr Lawson Tait, Dr Keith, and other great authorities on abdominal section might have to say. Un-

fortunately Dr Keith had been unable to be present, but from Mr Tait we have had a vivid and memorable *résumé* of a very difficult and complex subject. Abdominal section extends beyond the domain of gynæcology, which is by no means a purely surgical art. In the Liverpool Hospital for Women, to which he was attached, he found that out of 3500 women suffering from what might be briefly termed diseases of the womb, less than 1 per cent., only 0·7 per cent., required surgical treatment, even of the simplest character; and in Birmingham Hospital and similar institutions a like proportion obtained. The anxiety accompanying the performance of abdominal sections seems to loosen the tongues of some, though it renders others silent, and a large amount of minor gynæcology involving no surgery is got through by most of them without discussion. Mr Tait had led them through the intricacies and varieties of abdominal section, and now that he had been asked, through the courtesy of their President, to speak, he wished to point out the unlooked for promise of utility in some and the disappointing results in others. The surgery of the liver and gall-bladder was accepted by all as safe in execution and brilliant in its results, but the surgery of the intestines, in some of its aspects, was still rudimentary, doubtful, and obscure. Physicians hoped that when a typhoid ulcer had perforated the intestine the surgeon might be able to discover its locality, stitch up the rent, wash out the peritoneum, and save the patient; but, unhappily, it was difficult to find the ulcer, and as, when intestinal contents have entered the peritoneum, death is not due to peritonitis (of which there was often but little sign), the wisdom of surgical interference was dubious. In excision of the spleen for tumour and leucocythæmia surgery had also failed to establish its value. He had consulted nearly twenty text-books upon this subject, and the physicians all admitted they could do little or nothing. Such tumours were not very common; if each author had watched five, there were one hundred cases treated without a single cure having been effected. They looked to the physiologists to help them out of that humiliating dilemma. In the surgery of the female genito-urinary system the renown of ovariectomy and its successes were pre-eminent. It needed no expert to inform the public that an ovarian tumour, if left alone, was almost inevitably fatal; and once assured by the work of Sir Spencer Wells, of Dr Keith, of Mr Lawson Tait, and others, of the safety of ovariectomy, its acceptance by the public and by the profession was universal. But inflammation of the bowels; to use the popular phrase, and its sequent troubles were more difficult of apprehension, and not even the life-long work of Bernutz had settled the part played in the etiology of these painful and dangerous conditions by the inflammatory and suppurative diseases of the ovaries and their ducts. Until the pathology of Bernutz had been apprehended by the public, and acquiesced in by the medical profession, the proper treatment of that class of cases

would probably continue to be the subject of scientific discussion. The surgery of the kidney had been far more successful than could have been anticipated. Thornton, Tait, Morris, Lucas, and others, have shown that nephrectomy might yet outrival ovariectomy, and that suppurative and calculous kidneys admitted of satisfactory surgical treatment. Within twelve months he had removed a sarcomatous kidney, thoroughly emptied and drained a hydatid kidney that was as large as the uterus at full term, emptied and drained a renal abscess that was nearly as big, and removed a renal calculus. All had done well, and in the only case where he refused to operate, the right kidney being an enormous cyst with numerous calculi, the left kidney was shown at the post-mortem examination (as he had anticipated) to be no larger than a hazel nut.

Dr Berry Hart said he rose merely for the purpose of getting some information from Mr Lawson Tait, as they had him there that night. The first thing he would remark on was this—he thought it was to be deprecated that, in the cases with which Mr Tait's name was associated, they should operate from symptomatology, and not from physical signs. There was also a class of cases where the mischief spread through the connective tissue, and these would not be so favourable for operation as those where it passed through the Fallopian tubes. He was afraid that if they limited the examination of patients to symptoms, they would lose a great deal of information. He should like to know how the removal of the appendages benefited where there was peritonitis, and whether Mr Tait held that the removal of the ovaries and tubes exercised some influence on the nervous system. The last point was in relation to Listerism. He did not think Mr Tait had altered the principles of Listerism by his work, but he had shown them that they could carry out these principles by the sterilization of their instruments. For instance, from Koch's experiments, they knew now that boiling was perhaps the best way to render any subject aseptic; and a surgeon, by boiling his instruments and washing his hands thoroughly clean, carried out Lister's principles. And it was possible they had been wrong in carrying out Listerism by using such strong antiseptics.¹ In conclusion, he expressed the

¹ During the debate I spoke inadvertently of boiling sponges as a means of disinfecting them. Mr Tait has kindly forwarded me the following description of how he treats his sponges:—

“New sponges are first put into a large quantity of water with sufficient muriatic acid to make the water taste disagreeably acid. They remain in this mixture until all effervescence has ceased and all the chalk is removed. For this purpose it may be necessary to renew the acid several times. The sponges are afterwards carefully and thoroughly washed, to make them as clean as possible, and free from every rough particle. After being used at an operation, they are first washed free from blood, and then put in a deep jar, and covered with soda and water (1 lb. of soda to 12 sponges). They are left in this about 24 hours (or longer if the sponges are very dirty), and then they are washed perfectly free from every trace of soda. This takes several hours’

great pleasure with which he had listened to Mr Tait's opening address, and the very great benefit which they had all experienced from his writings.

Dr Cotterill said he would not have presumed to interrupt the discussion, but that he was in possession of an interesting specimen, which he thought Mr Tait and others interested in abdominal surgery might be glad of an opportunity of seeing. The case was admitted to the Infirmary a few nights ago, and showed symptoms of acute intestinal obstruction. There was peritonitis, accompanied with faecal vomiting. A tumour could be felt per anum, and a diagnosis of volvulus made. After the administration of chloroform this state of matters was apparently improved, and the patient had a faecal evacuation; but the symptoms returning, Dr Cotterill performed abdominal section, and in drawing up the colon was aware of something like a band giving way. This was followed by another faecal evacuation. There was a solid tumour in the pelvis fixed to the pelvic walls, presumably a sarcoma. Perforation of the gut had taken place, and there was faecal matter lying in the peritoneal cavity and evidences of local suppurative peritonitis. The patient died eighteen hours after, and at a post-mortem examination this unique state of matters was discovered:—There was, at the lower end of the sigmoid flexure, an annular tumour, two inches in breadth, surrounding the gut, and situated five inches above this a tumour, which he believed to be a papilloma, attached to the mucous surface by a pedicle about five inches in length. The tumour was then very much larger than they saw it now. During the act of evacuation, two days before death, this had come down and acted as a plug, with the result that the sarcomatous tumour had given way, and the patient had suffered from perforation of the intestine and peritonitis.

Dr Byrom Bramwell said that, as Mr Tait had well remarked, the section of the abdomen was a territory on which the physician and the surgeon met; and he hoped, therefore, it would not be thought out of place if he made one or two remarks with regard to that particular part of the subject which related to the physician. He would like, in the first place, to ask Mr Tait for the reasons which had guided him in coming to the very strong conclusion which he had expressed with regard to operations on cancer of the stomach—he alluded, of course, to the operation on cancer of the pylorus, putting out of account tumours which in-

hard work, using hot water, squeezing the sponges in and out of the water, and changing the water constantly. Leaving them to soak occasionally for a few hours in very hot water greatly assists in the cleansing. When quite clean, they are put into a jar of fresh water containing about one per cent. of carbolic acid, and, after being in this for 24 hours, they are squeezed dry, and tied up in a white cotton bag, in which they are left hanging from the kitchen ceiling (being the driest place in the house) till they are wanted."

volved the body of the stomach or the cardiac orifice. Cancers of the pylorus might be extremely limited. We all know that medicinal treatment affords no hope of cure, and hence it had occurred to surgeons to remove the pylorus. Now, he understood Mr Tait to object to this operation because it was for cancer, not because of the difficulty or danger of the operation itself. Mr Tait's brilliant results had shown that it was almost as safe to perform an operation in the abdomen as it was on an external organ, so far as any risks of a fatal result from peritonitis was concerned. Now surgeons, he took it, would hardly agree that all cancers—as cancer of the breast for instance—were not to be touched. That was a proposition which he supposed would hardly be admitted, and if the success of ovariatomists had shown that an operation in the abdomen could be as successfully performed, was it not right under such circumstances to place the chances for and against the operation before the patient for his decision. Would it not be right, therefore, if they were to operate on cancer at all, to operate in such a case as that, or at all events to give the patient the benefit of the doubt? He certainly would live for some time if they did not operate; but many people rather than die a slow and lingering death would run the great risk of this operation. He asked this question as seeking for information, not as an advocate of the operation, because it was occurring every now and again to physicians. In regard to intestinal obstruction, that, as Mr Tait had said, was an extremely difficult matter, which no one felt he could give any general rule about. He was disappointed that Mr Tait had not given them the rules which guided him in forming a decision on this matter. Mr Tait said they should operate when there was evidence of purulent fluid in the cavity of the abdomen. But, as matter of fact, in cases of obstruction it was seldom that they could detect such in the cavity; and if that was the only rule—

Mr Tait.—That I said of suppurative peritonitis, not of obstruction.

Dr Byrom Bramwell said he would much like to know what were the rules which guided Mr Tait's procedure in these cases. Professor Annandale had said he would operate in acute cases where the symptoms lasted over twenty-four hours. He took it that in many of these cases the surgeon was called in after the physician had seen the case in all its stages. For his own part, he would, speaking generally, hesitate to recommend the operation if the obstruction had only lasted twenty-four hours, and if the ordinary medicinal remedies (opium and belladonna) that they knew in many cases were successful had not been tried. What seemed to him the great difficulty in many of these cases was the question of diagnosis; they could not put all the cases together and lay down any definite rule applicable to them. One had to make careful inquiry into the history and the circumstances of the case, and endeavour to

determine what was the special cause of obstruction in each instance, and to be guided by that in forming his opinion. A case occurred, for instance, in which a man had been afflicted with malignant disease where there had been no symptoms. He was suddenly seized with obstruction, and perhaps died of peritonitis. He had known such cases, thought to be due to a band or some other mechanical cause. When a patient came before one in *extremis*, more especially in the hospital, and in cases in which there were no friends, it was not always possible to elicit all the facts; it was the difficulty of diagnosis that one had to contend with. As Professor Annandale had suggested, would it be right under those circumstances to attempt an exploring incision? He could not pretend to give any opinion, but he had always hitherto been under the impression—it was perhaps considerably changed by hearing of Mr Tait's brilliant works that night—that to open the abdomen in cases of acute obstruction was one thing, and that to open a healthy peritoneum was another. He should like to ask Mr Tait whether he advocated the opening of the peritoneum in these cases; whether he would expect to get the same results as in ordinary cases; whether there would not be special difficulties and dangers? Then there was another point which Dr Imlach had alluded to—the operation for perforating ulcers of the stomach, for removal of leucocythæmic spleens, and other cases of a similar kind. The great difficulty that a physician had in recommending, or a surgeon in performing an operation, was that when he was called to see the patient he was, in cases of perforation of the stomach, in such a state of collapse that the chances were that if the abdomen were opened the patient would die on the table. The same argument held in regard to leucocythæmic spleen. It was only in the last stages that surgeons had attempted to remove the tumour, when a slight loss of blood was of great importance. Possibly the brilliant operation which Duncan and Cotterill and others had performed of injection of blood had removed this source of danger; and it seemed to him to be the question for the future, whether in cases of perforated stomach or enlarged spleen it might not be possible, having infused blood and done away with the chance of collapse, to treat these cases more successfully than hitherto had been the case.

Dr Affleck said he wished only to interpose in the debate for a very few moments, to refer to one point which Mr Tait had invited the attention of physicians to, and which had been alluded to by Dr Byrom Bramwell, in regard to surgical interference in cases of intestinal obstruction. He agreed in the main with Dr Byrom Bramwell, but there were some points he wished to particularize, on which he held, perhaps, a slightly different opinion. The cases of acute intestinal obstruction were very varied, and the difficulties in diagnosing very great; and they knew there were forms which were remedial by very simple means,

and other forms that, humanly speaking, were incurable, except by interference on the part of the surgeon. The question was, When could that interference be advantageously undertaken? Even some of the most formidable forms of intestinal obstruction might right themselves. A twist of the bowel might relax itself; it was possible that the portion of an intestine getting into a band might get out of it again. Therefore, surgeons were very properly unwilling to interfere until the time when it was clear that no efforts could remove the difficulty. He spoke in reference to a case in his own ward, within the last fortnight or three weeks, of acute intestinal obstruction, where an abdominal section was made, but made, he thought, too late. The case was treated by the medical man and himself outside; and after all ordinary means had been used to try and get relief, he came to the conclusion that there was in all probability a band or other source of mechanical obstruction, and took her into his ward to have her examined by a surgeon, with the view and hope that the operation might be promptly performed. The case was seen by two of his colleagues, one of them being Dr Keith. There was some debate on the case, and several doubts arose. The temperature began to rise; the pulse got quick; signs of peritonitis set in. The operation was well performed. Two bands were found—one on the large, and one on the small intestine—which were cut, and the patient had an evacuation after—but died, apparently from the shock. He thought the operation had been too late, for if peritonitis set in in a case where intestinal obstruction seemed to be acute, and if to that was added the shock of an operation, the chance for the patient was a very slender one. He thought the time to interfere was before peritonitis set in, and the great difficulty was what Dr Byrom Bramwell had pointed out. He thought that after due consideration and exhaustion of the ordinary methods to remove the difficulty, when the conclusion was formed that it was in all probability a mechanical obstruction, no time should be lost in making an exploratory incision with the view of removing the obstruction.

Dr Foulis said that in the *Edinburgh Surgical and Medical Journal* for 1846 there was described an operation for ovariectomy performed by Dr Handyside in presence of Dr Hughes Bennett and several other gentlemen. This operation was performed on 5th December 1845 by Dr Handyside. The patient lingered on for seventy days, and died at last. She was a young girl of 21 years of age. He believed that was one of the first operations for ovariectomy ever performed in Edinburgh. But the point he wished to look at was this, that having described the operation and after-treatment of the case, Professor Hughes Bennett stated that in sewing up the abdomen, great care was taken *not* to include the peritoneum in the sutures. From that day to this enormous changes had taken place in the treatment of the peritoneum. No

one feared to handle the peritoneum in the most violent manner, and he ventured to say that a deal of the success commenced from the day that people took care to include within the sutures the peritoneum. There were one or two points as to tumours that he should like to bring before Mr Tait. One of the first disagreeable facts as to ovariectomy was that many of the patients who recovered returned in the course of one, or two, or three years suffering from malignant peritonitis, and Dr Thomas Keith was the first to state that he was almost disheartened from the study of ovariectomy, by the fact that many of the patients died of cancer within eight or ten years from the time of the operation. Why was that? Mr Tait had hit the nail on the head when he said that the secret of success lay in performing the operation as soon as it is possible to detect the tumour. The old plan was to leave the tumour to as long a date as possible, until the abdomen got quite accustomed to the tumour, and then operate. Had they any method of recognising the malignancy of abdominal tumours? He believed Mr Tait would bear him out in saying that almost every malignant tumour was surrounded by ascitic fluid. He was the first to point out in the *Edinburgh Medical Journal* the importance of examining the fluid microscopically before operating. If the tumour was allowed to grow to a large size, there was great danger of malignancy. There was such a proliferation of the connective tissue elements in the ovarian stroma, that the tumour might burst and the connective tissue elements might turn into sarcomatous elements, and affect the peritoneum, otherwise healthy. Hence the importance of recognising the nature of the tumour as soon as possible, and removing it. If, on tapping, the ascitic fluid was found to be bloody, it aided very much the question, whether malignancy existed in the abdomen. He believed a microscope would discover whether malignancy was there or not. He maintained that a careful microscopic examination of the fluid would enable a surgeon to avoid that very unfortunate result which so frequently happened after attempting to remove a malignant tumour of the ovaries. He would have great comfort from hearing Mr Tait state his views as to the question of the ascitic fluid surrounding the abdominal tumours.

In reply, *Mr Lawson Tait* said that he thought the best plan would be to take the points in the reverse order of the speakers. He would at once dismiss what Dr Foulis had said by expressing an entire concurrence in his view. There could be no doubt at all that the delay in the performance of ovariectomy favoured the recurrence of cancer after the operation, and that when ascitic fluid was found outside an ovarian tumour it meant one of three things,—the first, and by far the most probable, being cancer; and the second and third, of much less frequent occurrence, being either a rupture of the cyst, or suppuration of the peritoneal cavity. Dr Branwell had asked for detailed reasons for Mr Tait's

views against operations for cancer. This was very much too large a subject to go into, but he could only again express the belief already given at some length in the paper, that operations performed for diseases which preclude anything like permanently successful results were not such as redounded to the credit of surgery, or the advantage of the patients. Dr Bramwell had asked Mr Tait for rules to govern surgical interference in cases of intestinal obstruction, but that was precisely the question which Mr Tait had been asking Dr Bramwell and his fellow-physicians, as to whether it would not be possible for the men who were charged with these cases early in their progress to come to some kind of conclusion by which they could be guided towards permitting the interference of the operating surgeon. Dr Berry Hart and others seemed to have got a wrong impression about Mr Tait's method of dealing with his sponges. He certainly never boiled them, for boiling a sponge ruined it, and sponges were a very costly item, particularly in his line of practice. He could perhaps answer Dr Berry Hart and Dr Halliday Croom in the same way concerning the symptoms and signs by which he was led to interfere in cases of chronic inflammatory disease of the uterine appendages. He certainly was always extremely loth to interfere unless he could obtain absolute evidence on physical examination that the disease was local, and thereby justifying the view that the symptoms were not merely of a general and purely subjective kind, and this he rarely found much difficulty in ascertaining by the single finger examination. It is perfectly true that from time to time it became necessary to use the bi-manual method, and even an anæsthetic, but these were the doubtful cases. Unless completely assured by a responsibility shared with other people to assure him that everything was done to protect the patient, he did not operate in the absence of physical signs; but, of course, it must not be forgotten that in this, as in every other department of medicine and surgery, the patient must have some kind of vote in the matter, and, therefore, he always carefully explained to his patients what conditions were probable in each case, and what might be done to alleviate them; and the patients, in every instance, had the choice as to what method of treatment should be adopted. In such a line of practice as his, this kind of proceeding was essential, for there were many patients who would rather go on suffering than take any kind of risk; whilst on the other hand there were others—and by far the more numerous class—who would submit to anything like a reasonable risk for the sake of getting rid of perpetual ill-health. It was in this way that he could answer Dr Croom's question as to where he drew the line. The line was not drawn by him; it was drawn by the patient and her friends and advisers in each individual case, and it must not be forgotten that this line was a movable thing, the social condition of the patient governing the position of the line in a very remarkable

way, for an operation which was hardly to be justified in the upper ranks of life might become an imperative necessity in the case of a governess or a servant girl. He was delighted to find his old friend Dr John Duncan had come to a conclusion pretty much the same as his own; and although he differed from Dr Duncan when he said that Listerism would never die, really Dr Duncan had succeeded in slaying Listerism completely that night. He had apparently given up all belief in aerial germs, and regarded only germs that could be carried into the wounds by the surgeon's own fingers and instruments. This, doubtless, was a great step in advance, for we had long been blaming outside conditions when really the blunders were to be laid at our own door. This was a new kind of *ism*, which he would venture to call *Duncanism*, and it very much resembled what for years he had practised, which might be called *Taitism*. The questions raised by Dr Maclaren and Professor Simpson were of extreme interest, but he was bound to say that he approached them with a considerable amount of hesitation, for being in a position of a purely special kind, whatever opinions he might express would always be regarded as based in that direction. But he could not help feeling that in his own case an enormous mass of material was being wasted, as far as purposes of instruction were concerned. He had very few pupils, and in the ordinary sense of pupils he certainly did not desire to have them, for it would be perfectly impossible for him to undertake to devote his time to the teaching of the elementary facts of gynæcology, and to teach a large number of students the advanced practice of that department of our art was throwing instruction away. He supposed that every year the University of Edinburgh turned out about 200 graduates, and he ventured to say that to 180 or 190 of these such practice as he was engaged in would never be of any kind of value at all. To the other ten or twenty it might, on the other hand, become the object of their lives. What was really wanted for instruction in specialism was the construction of a post-graduate school. Such a school might be partly situated in Edinburgh, partly in London, and partly in Birmingham, or elsewhere. It was perfectly certain that what was wanted was a co-operation of leading men in the various specialties of medicine and surgery, by which such pupils as desired to have a complete, or more or less complete, course of instruction in any of the specialties might secure what they wanted. As matters stood, it was only possible for a man after his graduation to receive pupil's instruction, whilst he wanted much more; whilst, on the other hand, it became a matter of necessity for each general practitioner to undergo a systematic course of instruction in specialisms which he would not use except to a very limited extent. The kind of specialism required by the general practitioner was merely the selection of a limited number of striking features upon which most serious blunders might be made; such, for instance, and very

often in discussing the question Mr Tait had taken this as a brilliant example, the symptoms of acute glaucoma, a point upon which but few months pass in the practice of any ophthalmic surgeon without some terrible mistakes being met with. In conclusion, Mr Tait need not say that the gratification which he experienced from the magnificent reception which he received, from the kind allusions to his own work, and from the brilliant assembly before him, was altogether too great for utterance in words.

The President of the College of Surgeons then said he rose not to take part in the discussion of abdominal surgery, in which case he would be entering on a subject which was entirely out of his sphere, but, on the other hand, to ask them to accord a most hearty and cordial vote of thanks to the College of Physicians for the use of their hall for that meeting. That large meeting which had gathered to hear the address of Mr Tait would have been very insufficiently accommodated in the hall in which the Medico-Chirurgical Society usually met, and he was sure much of the pleasure which they had enjoyed that evening would have been lost there, for they would have been inconveniently seated on hard chairs and overcrowded, and would with difficulty have heard the interesting address, and not less interesting discussion by which it was followed. Therefore, he called them to cordially pass a vote of thanks to the College of Physicians for the use of their elegant and commodious hall for that meeting.

Dr Littlejohn seconded the motion, which was carried with enthusiasm.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XLVII.—MEETING IV.

Wednesday, 10th February 1886.—*Dr HALLIDAY CROOM, President, in the Chair.*

ON the motion of the President, seconded by Professor Simpson, it was resolved to adjourn the meeting for a fortnight, as a mark of respect to the memory of Dr Angus Macdonald, whose death occurred that morning.

MEETING IV. ADJOURNED.

Wednesday, 24th February 1886.—*Dr HALLIDAY CROOM, President, in the Chair.*

I. *The President* read an OBITUARY NOTICE of the late Dr Angus Macdonald, which appeared at page 995 of this Journal. It was resolved to send a copy of the President's notice to Mrs Macdonald.

II. *Professor Simpson* showed the HEAD AND CORD of a child born in precipitate labour.

III. *Dr Milne Murray* read a paper on SOME OF THE PHYSIOLOGICAL AND THERAPEUTIC EFFECTS OF WATER AT DIFFERENT TEMPERATURES, which will appear in a future number of this Journal.

Professor Simpson said that the interesting paper which they had just listened to was so superlatively good that he would begin by making a criticism. He was sorry to find that *Dr Murray* had said little or nothing as to the literature of the subject, and he was sure the Society would have appreciated a brief summary of what had been already done in this connexion. He himself missed this much, and hoped it might be rectified before publication. He thinks the profession are greatly indebted to *Emmet* for his introduction of the use of hot water as a styptic, and though, as was pointed out in the paper, his explanation of its action was unsound, this did not detract in any way from the great value of warm water as a therapeutic agent. The observations just recorded seemed to him to be of the very greatest interest, and the practical deductions seemed sound and reliable. He was interested to observe that it was noted that during contraction the uterus became pale and bloodless. The vascular condition of the uterus during contraction has been the subject of discussion, and there is still doubt as to the condition of the circulation during tonus. *Lahs* and *Küneke* have different views on this point, the former holding that the blood is driven out of the organ, while the latter holds that during this time the sinuses contain the blood, they being filled during tonus. This question is obviously one of importance. What became of the blood which, according to *Lahs*, is driven out of the uterine wall during a pain? He supposes it is largely directed to the cervix, and that this congestion of the cervix accounts for the softening changes which go on in that organ during labour. In connexion with this question *Dr Murray's* observations are of much value, for the pallor of the organ accompanying contraction would seem to confirm the view that the blood is driven from the uterus during tonus. He agreed generally with the deductions made from the experiments. He thought that that relating to the value of hot water as retaining the natural heat of the body to be important. We were apt to forget the great loss of heat, and consequently the diminished chance of reaction which must follow the use of cold water injection and application. The observation relating to abortion was also of much interest, and worthy of being kept in mind in dealing with such cases.

Dr Berry Hart said the paper was one which did not lend itself to criticism. The experiments described and deductions made were, in his opinion, of extreme value. For his own part he differed with *Professor Simpson* as to the absence of literature. The paper was so full of original and interesting matter that there was no room for "literature," and he was glad to find it had been left

out. He felt that some of their papers were overburdened with references to previous authors, and he was glad to find one so full of original observations. He was especially interested in the experiments relating to the checking of hæmorrhage by means of hot water. He intended in any abdominal section in which trouble arose from oozing, to pass hot water into the abdominal cavity, and see whether this did not give satisfactory results. He had listened to the paper with much pleasure and interest, and hoped soon to be able to study it more carefully in print.

Dr Leith Napier said that he noticed that in speaking of the action of hot water in controlling gastric hæmorrhage, Dr Brunton stated that one obtained more satisfactory results by taking the water in sips than by drinking large quantities of it at once; and he was himself inclined to think that this was the case. He had employed the hot douche for hæmorrhage for a considerable time now, and had found it a most convenient and efficient hæmostatic. He did not think it possible to make sure of the proper temperature without the use of a thermometer. The common pocket instruments were graduated to 115°, a usually sufficient temperature.

Dr M'Raild agreed with Professor Simpson that the paper would have been more complete and satisfactory had a short account of the literature been appended. He thought the title, too, erred on the side of vagueness. There was one question which had scarcely been referred to by Dr Murray, and it was one of much interest, namely, What was the theory to account for the manifest difference in the action of hot and cold water on non-striped muscle? The question was one, no doubt, of great difficulty, but at the same time it was of much importance. The effect of the hot water depends not merely on the heat, but on the moisture plus the heat. A hot plate, for example, could not have had the same effect. The action was thus, it seemed to him, what might be called "cataplasmic."

Professor Chiene (on the invitation of the President) said it had given him much pleasure to be present and hear this important paper. He was glad to have been present to hear the correct explanation of the action of hot water in controlling hæmorrhage. The subject was one of great interest to surgeons, and he noticed that Dr Murray had regarded their neglect of this hæmostatic as a reproach to them. He thinks that one reason of the neglect which it has met with among surgeons is that in operations in such places as the mouth, where the difficulty in controlling hæmorrhage is very great, there is a risk of the water running into the air passages, and so increasing the danger of the operation. He had had several conversations regarding the matter with Dr Murray, and he had quite recently had two opportunities of putting the matter to the test. The first one was a case of excision of the tongue where the hæmorrhage was severe. In this case the action

of the hot water, applied by means of sponges, was only partially effective, and he believed this was because it was not used hot enough or freely enough. In the other case, one of excision of the breast, there was a great deal of troublesome oozing from vessels connected with the ribs, and which could not be secured. Hot water was freely applied, the tissues became instantly blanched, the hæmorrhage ceased, and the wound was closed, scarcely a vessel having been ligatured. The patient had done perfectly well. This use of hot water reminded him of the practice of the late Mr Spence, who made it an invariable proceeding to pour from a kettle a quantity of hot water upon all flaps of stumps and other large wounds. He (Professor Chiene) had often noticed the blanching and pallor which followed this application, and he had no doubt it greatly tended to diminish primary oozing and reactionary hæmorrhage. He had been present with much pleasure, and had learned a great deal from the paper.

Dr Sinclair said that when he read the title of the paper, and came into the room and saw the wall covered with diagrams and tracings, he was inclined to ask *Cui bono?* But he was greatly pleased to find that the paper was an eminently practical one, and he was glad he had had an opportunity of hearing it. He had been of opinion up till now that the action of hot water in checking hæmorrhage was due to its effect in coagulating the blood, and thus, though he had been convinced of its efficiency in controlling hæmorrhage, he had felt some hesitation in using it, in view of the danger which might arise from the coagula formed. He was, however, glad and relieved to find from these observations that it acted as a true astringent, affecting the muscular coats of the bloodvessels, and not as a coagulant.

Dr Horsley asked whether the use of cold water first in cases of hæmorrhage while risking failure in controlling it, might not also render the action of hot water less efficient afterwards.

Dr Underhill had often been in doubt whether to use hot or cold applications in the treatment of post-partum hæmorrhage. He had always employed cold applications, and had always succeeded in bringing about contractions, and in thus checking the hæmorrhage. But he had often been struck by the great amount of feebleness and exhaustion which followed the use of cold applications to the vagina or abdomen. He had no doubt that the explanation lay chiefly in the amount of heat abstracted by the cold water, etc., and that the patient's already enfeebled vitality was further greatly reduced. There was further often a noticeable tendency on the part of the bleeding to recur after the use of cold water, and he had no hesitation in accepting the experiments as offering a correct explanation of the difference between hot and cold water in this respect. He had followed the reading of the paper with very great interest, and he felt bound to say that he had never before listened to a paper in the Society

which lasted upwards of an hour in reading without feeling that he had had enough of it, but in this case Dr Murray had succeeded in securing their interest from the beginning to the end.

Dr Spence had had pleasure in listening to the explanation of phenomena with which he had been long familiar. He had for long employed hot water as a styptic in bleeding from a broken alveolus, crushed finger, etc., and had never found it to fail. He was, however, glad to find that in this practice he was going on strict physiological lines, and it tended to increase his confidence in the practice. He had often noticed how freely a wound inflicted while bathing tended to bleed, and how difficult it was to stanch such. This bore out the view that prolonged cold tended to increase and not to check hæmorrhage.

Dr Barbour agreed with the previous speakers as to the value of the contribution. There was one point on which he would have liked some information, namely, as to whether it was supposed that the action of these stimuli was due to effects on the muscle or nerve. He did not think this had been touched upon in the paper, but he regarded it as a matter of great interest; and he would be glad to hear what view Dr Murray took regarding it.

The President did not feel justified in adding anything further to what had been said. He had listened to the paper with great interest and pleasure, and agreed with everything that had been said as to its practical and theoretical value.

Dr Milne Murray, in reply, thanked the Fellows for their kind reception of his paper. With regard to the matter of literature, he wished to explain that as his paper was an unusually long one, he did not feel justified in taking up the time of the Society with an account of the literature then, but meant to give a *résumé* of it when the paper appeared in the Transactions. He was of opinion that during a uterine contraction the uterine vessels became almost empty of blood, and this was further confirmed by some observations he had made by means of the arteriograph, which showed that there was both a rise of blood pressure and pulse rate during active uterine contraction. In reference to Dr Hart's remarks, he was satisfied that hot water would prove an efficient hæmostatic in abdominal surgery. He had found that it was efficient in stopping the escape of blood from very large vessels in the rabbit. With regard to determining the proper temperature, he was of opinion that a thermometer, though perhaps convenient, was not essential. He had found that water in which the hand could just be held was at a temperature of from 115° to 120°, and that this was the most convenient temperature to use. We should remember that the mucous membrane is much less sensitive to heat than the skin, and that we can drink water at a much higher temperature (135° to 140° F.) than we could bear to hold the hand in. Thus if water tested by such a means is employed, we need apprehend no danger to the tissues of the vagina

or other organs. To protect the buttocks from the water running out, it was a good plan to dip a towel in cold water and fold it close up against the lower hip and vulva. This rendered scalding of the external parts impossible. In reply to Dr M'Raild, he was sorry he could not venture on a very definite expression of opinion as to the explanation of the difference on the action of the different temperatures. He was inclined to think that the high temperatures induced an active molecular change in the muscular tissue, and such a change would be less likely a result of the application of water at low temperatures. He was at present investigating this matter, and hoped to reach some solution of the question by-and-by. He was very glad to hear Professor Chiene's experience of warm water as a hæmostatic, and was sure it would come into wide use in surgery. There could be no doubt that its action was that of a true astringent, and not simply a coagulant, and there need be no dread of its involving risk from the production of coagula which might be carried into other vessels.

CARLISLE MEDICAL SOCIETY.

SESSION II.—MEETING VI.

Thursday, April 15, 1886.—Dr LOCKIE in the Chair.

Dr Forster showed a case of OCULO-MOTOR PARALYSIS, of seven weeks' duration. The paralysis was complete, and had come on suddenly.

Dr Smith showed a case of HODGKIN'S DISEASE.

Dr Helm read a paper on TUBERCULAR DISEASE IN CHILDREN.

Dr Lockie said that tubercular diseases were much commoner among the poor than among the better classes. Tubercular meningitis might have no initiatory rise of temperature. Later, irregularity and slowness of the pulse are important symptoms.

Dr Sydney Coupland said that it had been clearly proved that tuberculosis can be transmitted from one animal to another by tubercular matter only. The fallacies of experiments on the artificial production of tuberculosis were now acknowledged. Some people were more vulnerable to the tubercular poison than others, and this liability was transmitted. It was possible that children might be infected from animals; as from the milk of tubercular cows. In tubercular meningitis there was often oculo-motor paralysis to a slight extent. Tubercle in the choroid was comparatively rare; optic neuritis more frequently present.

Dr Macdougall said that it was important in tubercular children to attend very carefully to scrofulous glands, enlarged tonsils, ozæna, discharge from the ears, and such like. He had noticed in several cases of tubercular meningitis a certain amount of muscular contraction in the back of the neck, accompanied by severe pain.

Dr Maclaren believed that tubercular meningitis was not always fatal, inasmuch as in some fatal cases there was a history of previous attacks with tubercular symptoms.

Dr Lediard said that in many cases phthisis followed the presence of strumous glands in neck. The bacillus of tubercle was very persistent in phthisical sputum.

Dr Brown showed a HEART FROM A CASE OF CONGENITAL CYANOSIS. There is great hypertrophy of right ventricle, and comparative smallness of left ventricle. There is deficiency of septum ventriculorum at its base. The orifice of the aorta overhangs and freely communicates with both ventricles. The aortic valves are normal. The pulmonary artery arises from the right ventricle to the right of the aorta, and is much contracted. The semilunar valves are situated half an inch from the orifice. There is no communication between the auricles, and no ductus arteriosus.

PATHOLOGICAL SPECIMENS.

Dr Brown.—ROUND-CELLED SARCOMA OF TESTICLE.

Dr Smith.—ENDOCARDITIS AND ENDOARTERITIS.

Dr Lediard.—FIBROUS TUMOUR OF BREAST; EPITHELIOMA OF LABIUM; MOLLUSCUM FIBROSUM.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS

By WILLIAM CRAIG, M.D., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine, etc., etc.

THE USE OF NITRO-GLYCERINE IN DIFFERENT FORMS OF NEPHRITIS.

Dr F. P. Kinnicutt has continued his studies on the use of nitro-glycerine, and formulates his conclusions from them as follows, in a paper read before the New York Academy of Medicine at the meeting held on Feb. 16 (*New York Med. Journ.*, 20th Feb. 1882):

1. That in nitro-glycerine, given in small doses frequently repeated, we possessed a powerful agent for lowering increased blood-pressure which was very commonly associated with uræmic symptoms.
2. That its power to control or relieve many of the paroxysmal disturbances of the nervous system included under the general term uræmia, headache, and asthma especially, was more marked than that of either opium or chloral.
3. That its action in parenchymatous and interstitial nephritis was to increase the amount of urine excreted, and to diminish the quantity of albumen.
4. That in nitro-glycerine we had the means of maintaining more or less continuously lowered blood-pressure, thus arresting or

relieving symptoms and prolonging life. Regarding the administration of nitro-glycerine in chronic nephritis, the strength of a single dose should be just within the limits of producing subjective symptoms, and this could be determined only by careful trial in individual cases. It was well from time to time to increase a single dose to the production of subjective symptoms to learn whether the system had not become tolerant from its long use, and, if found necessary, to increase the dose. Dr W. H. Draper not being able to be present, but knowing the contents of Dr Kinnicutt's paper, sent some notes, which were read by Dr W. M. Carpenter. The writer had had many opportunities for observing the efficiency of nitro-glycerine in relieving uræmia, headache, and asthma. There could be no question, judging from his experience, of the very prompt and decided relief which nitro-glycerine gave to these distressing symptoms, and though it sometimes failed where opium succeeded, it was certainly to be preferred to opium where it would give relief. In his experience it had failed in cases of advanced disease, the function of the kidney being well-nigh exhausted and the heart greatly enfeebled. Concerning the ability of nitro-glycerine to control the progress of interstitial nephritis he had no experience to offer. As to its power to diminish the amount of albumen, he thought a large number of observations were necessary to establish this result. He was not prepared to say that nitro-glycerine as a cardiac stimulant possessed the advantages over alcohol, opium, and digitalis, alleged for it by Dr Burroughs. Dr C. L. Dana's experience with nitro-glycerine had been chiefly in the treatment of nervous disorders, but recently he had had occasion to employ it in six cases of renal disease. He had also administered it to five persons in health to determine its diuretic effect, and in all it had rapidly and decidedly increased the excretion of urine, but it had to be given in considerable amount. In two of the cases of kidney disease, with acute symptoms, probably ingrafted upon a chronic condition, nitro-glycerine did not cause a diuretic effect. When given in large doses the drug caused an increased amount of urine, which continued to increase for two or three days after stopping its use, and then began gradually to diminish. This was evident, he thought, from the charts presented by Dr Kinnicutt, as well as from his own cases. Dr Kinnicutt called attention to the unreliability of some specimens of nitro-glycerine, and said it should always be tested by producing subjective symptoms.—*Therapeutic Gazette*, 15th March 1886.

QUILLAJA SAPONARIA AS AN EXPECTORANT.—We have already, in a previous number, referred to Kobert's proposition as to the substitution of quillaja bark for senega; and Dr F. Goldschmidt reports, in the *Zeitsch. für Ther.*, 15th December 1885, the result of a series of experiments which have been made with this drug. The mode of administration was to make a decoction of 75 grains

of the bark in 4 ounces of water, to which 5 drachms of syrup may be added. Of this mixture a tablespoonful may be given every hour. Quillaja was employed in thirty cases, invariably with favourable results in cases where the expectoration was difficult, such as in high degrees of emphysema, dilatation of the bronchi, and chronic bronchial catarrh. After the use of quillaja the expectoration always became more copious and easier, and the respiration freer. It therefore appears that quillaja possesses the power, in a very marked degree, of rendering expectoration easier. Contrasted with the other expectorants, it is well known that the ammonia alkalies, apomorphine and ipecac, when employed for any length of time, nearly always occasion such marked digestive disturbance as to entirely prevent their use. Senega in its mode of action, and in the results which it produces, is closely comparable to quillaja. Both are able to start suppressed secretion, and to aid the removal of it when profuse. The advantages of quillaja over senega have already been alluded to. The mode of action of quillaja appears to be twofold. It not only increases the vigour of the cough, and so facilitates the removal of secretion, but it also renders the secretion more fluid, and so enables its removal with greater readiness.—*Therapeutic Gazette*, 15th March 1886.

ON IODOL.—Dr Georg Banno Schmidt reports, in the *Berliner Klinische Wochenschrift*, on the advantages of iodol as ascertained from the use of this antiseptic at the surgical clinics of Heidelberg. Iodol, or tetraiodopyrol, has a light-brown colour, and is almost odourless, in which respect it offers a decided advantage over iodoform. Unfortunately it is not very soluble in water, only in a proportion of 1 to 5000. In alcohol it dissolves in three times its weight. The drug is employed in the following manner:—1. As a powder, in the same manner as iodoform. Being a finer powder, it enters and covers the part more completely than the former. It forms no scurf (in opposition with iodoform) on the wound, renders the secretion odourless, favours the formation of healthy granulation, and has a general salutary influence on the nutrition of the affected parts. 2. As solution, in 16 parts of alcohol and 34 parts of glycerine, for the saturation of uterine or rectal tampons. It is also injected in this form into empty or scraped-out abscesses. 3. As gauze. 4. In combination with vaseline and collodium. In no instance, in spite of its extensive use, were any symptoms of intoxication observed, nor was ever any iodine after its use discoverable in the urine. It is probable that its action depends on the generation of free iodine, though we have no definite data yet regarding this point. At all events, its action is prompt and harmless, and offers no small advantage over the iodoform in the absence of any odour, though its poor solubility and its (at present) high price are equally great objections.—*Therapeutic Gazette*, 15th March 1886.

TREATMENT OF PNEUMONIA WITH INUNCTIONS OF MERCURIAL

OINTMENT.—The treatment of pneumonia with mercurial ointment, proposed by Barthel and Moritz, of St Petersburg, in the *St Pet. Med. Woch.* of 16th Jan. 1886, is, as a typically antiphlogistic measure, of a pretty old date, for as early as 1850 the mercurial treatment was recommended by Wittich as a check, and in opposition to the practice of French physicians, to the employment of abstraction of blood. Of course, the calomel which Wittich and his contemporaries prescribed was not exhibited in the antiseptic sense as the mercury ordered at present by the above-named Russian physicians. But no matter whether the metal is supposed to reduce the excess of fibrin, diminish congestion of the lungs, and favour resorption of the inflammatory exsudate, or whether we mean to kill by its action the specific etiological factors of the disease—Friedländer's pathogenetic microbes—the mercurial treatment of pneumonia is usually attended by good results. The success of iodoform and similar inunctions in a large number of peritonitic and meningitic affections induced Barthel and Moritz, chiefs of the Obuchow Female Hospital of St Petersburg, to try the inunction of mercurial treatment in croupous pneumonia. That mercury, if applied in the form of an ointment for the skin, was absorbed, of course, is a well established fact, and has been known ever since Voit's memorable publication, "The Absorption of Mercury and its Combinations in the Body," Augsburg, 1857. For the last two years this has been the only medication used in croupous pneumonia, and shows a favourable mortality rate, as compared with the records of former years. The mortality rate of 31·4 was lowered by the mercurial treatment to 6·2 per cent., certainly a remarkable difference. The inunctions are to be begun with at once when the diagnosis is established. Morning and night a drachm is to be used; in very grave cases 3 instead of 2 drachms may be used daily. The inunction is to be made on the extremities, abdomen, and back; the thorax is to be left exempt, as the skin in this locality is wanted intact for the water compress (the German routine treatment of respiratory affections *vide* Berlin letter of *Gazette*, Jan. 1886). There were used in one case 2 drachms of mercurial ointment, in three cases 3 drachms, in seven cases 4 drachms, in six cases 5 drachms, in one case 6 drachms, in one case 7 drachms, in seven cases 8 drachms, in three cases 9 drachms, in one case 10 drachms, in one case 12 drachms, in one case 13 drachms. In spite of directly ordered gargles of chlorate of potassium, the authors observed in some instances slight gingivitis and moderate salivation, though never a serious stomatitis or colitis. Alongside of these inunctions, the inevitable compress was applied on the thorax as intimated above, and digitalis and quinine given according to the indication of the case. In view of the very favourable record produced by this mercurial treatment, we can only support the recommendation by our Russian colleagues of mercury in croupous pneumonia. — *Therapeutic Gazette*, 15th March 1886.

PERISCOPE OF GYNÆCOLOGY AND MIDWIFERY.

By J. MILNE CHAPMAN, Inverness.

VARIATIONS IN THE COMPOSITION AND CHEMICAL REACTION OF THE FEMALE GENITAL SECRETIONS (Ménière, pamphlet).—The following are the writer's conclusions:—1. The human organism is essentially alkaline. 2. The female is less so than the male. 3. The vulvo-vaginal mucus tends to become more acid in proportion to the debility of the constitution; under the same circumstances the uterine mucus becomes less alkaline, the state of health corresponding with a slight acidity of the vulvo-vaginal secretions, except that of Bartholini's glands, and a slight alkalinity of the uterine mucus. 4. These changes of composition are proved by clinical facts, chemical analysis, microscopic examination, and more particularly by the cure of sterility effected by the use of certain natural wells. 5. Along with this change of reaction there exists hypersecretion, change of composition, and the appearance of micro-organisms; in all cases, however, the uterine mucus presents a field in which parasites, animal or vegetable, are never met with. 6. Notwithstanding these changes in composition, the secretions of the muciparous follicles and of the vagina always retain their acidity, while the mucus secreted by the follicles in the cervical cavity and in the body, and that of the vulvo-vaginal glands, remains always alkaline.

THE LENGTH OF THE FOOT CONSIDERED IN RELATION TO THE DEVELOPMENT OF THE FŒTUS (Negri, *Annali di Ostetrica*, 1885, p. 220).—The writer states that there exists a constant relationship in the above respect, and that when the foot attains a length of eight centimetres the fœtus will be fully developed, and will weigh at anyrate $7\frac{1}{2}$ pounds.

RECOGNITION OF DEATH OF THE FŒTUS BY ABDOMINAL PALPATION (*Ibid.*)—The author states that when the fœtus is dead, if the examiner, while palpating the superior pole of the uterus, gives a series of little blows with the tips of the fingers, a crackling sound resembling the crepitus of fractures will be noticed.

RELATION OF MENSTRUATION TO THE DEVELOPMENT OF THE FŒTUS AT TERM (*Ibid.*)—1. Children born at term of primipara who began to menstruate early in life are larger and better developed than are those of women who began to menstruate late. 2. The period of sexual life of the mother has also an effect on the size of the child,—thus children born at term of mothers advanced in their sexual life are less developed than are those born of mothers who are earlier in that respect. It is from the sixth to the tenth years of sexual life that women bring into the world the best developed children.

MICROBIOLOGICAL STUDIES OF THE LOCHIA (Fasola, *Ibid.*)—1. Micro-organisms can *always* be found in the lochia shortly after delivery. The most ordinary forms are mono- and diplo-coccus, a few bacteria, and more rarely bacilli, lepto-thrix, and stepo-thrix. 2. Where no prophylactic measures are adopted in normal cases, their number is small; it increases during the first days, and they disappear about the seventh or eighth. 3. Vaginal injections during labour result in their increasing more slowly. They are found in greatest numbers when the lochia are altered, but after intrauterine injections, their number diminishes in a very notable manner. 4. By the daily use of intrauterine carbolic injections their number remains small, even though the lochia are foetid. Sublimate injections keep the number still less.

TWO CASES OF THE EMPLOYMENT OF THE CONSTANT CURRENT TO INDUCE PREMATURE LABOUR (Carl Fleischmann, *Archiv f. Gynäk.*, Bd. xxvii., Hft. 1).—The first case was one of deformed pelvis. Labour was induced at the end of the eighth month. One electrode was placed on the uterus, the other in the vagina. Contractions continued several hours after the end of each sitting. Three applications were made daily, each lasting ten to twenty minutes. After the eleventh sitting the os admitted two fingers, and thereafter the electrode was passed into it. On the twelfth day of treatment the labour was terminated by forceps. In the second case similar treatment was conducted for eight days, and the case terminated without further assistance. He found, however, that much better contractions were stimulated when the positive electrode was placed over the lumbar vertebræ, and the negative in the posterior vaginal cul-de-sac. The writer believes that in the first case the contractions were largely excited by the mechanical irritation, but in the second, especially when the electrodes were placed as described, that the result was due to the electricity, and he recommends this method of employment as the most efficacious, and as less likely to cause burning of the tissues, as the vaginal electrodes can then be used of much larger size; and further, that the risk of infection is greatly diminished owing to the non-introduction of the sound-electrode through the os.

FARADIC ELECTRICITY IN RIGIDITY OF OS UTERI DURING LABOUR (Mary Putnam Jacobi, M.D., *American Journal of Obstetrics*, January 1886).—The following short article is of sufficient interest to bear reproduction:—A primipara was brought during a premature labour, occurring at seven months of pregnancy, to the N. Y. Infirmary in a state of considerable exhaustion resulting from the prolonged labour pains. The external os was tetanically rigid. I did not see the patient until after she had been for some time in the hospital, and the physicians in charge, Drs Blackwell and Cushier, had used all the most usual and approved means of relaxing the rigidity of

the os, but without the slightest effect. Even chloroform had failed, and the increasing exhaustion of the patient rendered this method hazardous to be persisted in. It seemed to me that the tetanized condition of the os, which would barely admit the tip of a finger, and resisted manual dilatation to an extraordinary degree, was precisely due to the exhaustion of the nerve force destined to the uterine fibre. The tetanus would then be analogous to the intestinal cramps of lead colic; to those induced in both the rectum and the genital canal by compression of the aorta (in rabbits), or, on an even more general scale, to the universal muscular contractions of rigor mortis. If this were true—and surely the clinical history of cases of rigid os uteri tends to support the hypothesis—local stimulation of the exhausted nerve fibres was indicated as the remedy. A small electrode was applied to the os, and connected with a faradic battery, the other electrode being held in the patient's hand. It was considered advisable to avoid passing the current through the body of the uterus, lest new contractions should be excited and struggle in vain against an impassible resistance. The application was continued for fifteen minutes. Immediately afterwards, and for the first time, Dr Cushier succeeded in inserting a finger into the cervical canal, and after some further effort in gradually effecting manual dilatation and delivering the patient by the forceps. Stimulus to the nerve fibres thus seemed to have succeeded in inhibiting the spasm into which the muscular fibre had been thrown, as is habitual when left to its own irritability.

ELECTRICITY IN MIDWIFERY (Blair, *American Journal of Obstetrics*).—From an extended experience of faradization in midwifery, the writer claims—1. That it may be used with benefit in the persistent vomiting of pregnancy. 2. That it is a remedy of great value in arresting permanently hæmorrhages which precede, accompany, and follow abortions. 3. That premature labour can be induced by its use in a few minutes at any time when it is indicated. 4. That it has great advantages over anæsthetics in the mitigation of the severity of labour pains. 5. That it stands unrivalled as an oxytocic. 6. That it is a *certain* prophylactic against post-partum hæmorrhage, and one of great value against septicæmia. 7. That it occupies the first rank in that class of remedies which are used to promote incohition when it is arrested from any cause whatever. Blair further claims that when properly used, that is, with neither electrode near the fetal head, electricity is unattended with danger to the child. He employs a mild interrupted current, just strong enough to be perceived by the patient, increasing the strength gradually if necessary. He records several cases to illustrate each of his propositions. As any reliable induction apparatus will serve, if Dr Blair's successes are corroborated by others, no obstetrical bag

should be without a portable battery. At present, however, they are not altogether confirmed. Bumm, after a series of experiments, has arrived at the conclusion that in the majority of cases the muscular activity of the uterus is not influenced sufficiently to justify us in regarding electricity as a useful therapeutical agent; and Dr J. Syromalmkov (*Brit. Med. Jour.*, Oct. 3, 1885) found, that after the use of an induced current, for the purpose of the induction of premature labour, at intervals spreading over a week, there was only sufficient dilatation to admit the introduction of a No. 1 Barnes' Bag.—*Birmingham Med. Review*, March 1886.

TREATMENT OF FIBRO-MYOMATA BY LAPAROTOMY (Koeberlé, *Centralb.*, 44, 1885).—The writer tabulates as indications for operation—1st, A critical condition of the patient, the result of profound anæmia or of rapid growth of the tumour. 2nd, Increase and rapid growth of the tumour in young women (the nearer the climacteric the less pressing is the call for surgical interference). 3rd, Origin of the tumour in the lower uterine segment or between the layers of the broad ligament, acute pains or decided inconveniences caused by the tumour. As contra-indications to operation, or as adding greatly to the risk of operation, he mentions extensive adhesions, wedging in of the tumour into the pelvis, rapidly increasing ascitis, and incurable or constitutional affections. The remainder of the paper is occupied with a description of the operator's technique.

SANTONINE IN AMENORRHŒA.—Mr Walter Whitehead, in the *Lancet* for 5th September 1885, records how by accident he noticed good results after the employment of 10 grain doses of the above drug on two consecutive nights, and says that since then he has given the drug empirically to all patients who have come under his notice suffering from amenorrhœa "with expectant uncertainty." Dr A. Routh, in the same journal for 9th January 1886, records that he has tried this treatment in twenty cases; in all he produced the full physiological action of the drug, but he neglected to give the morning saline purgative used by Dr Whitehead. His experience is that the treatment is useless in chlorotic cases, but that it seems to have some power in healthy, full-blooded women; and as these latter cases are usually the more obstinate, the experiment is worthy of further trial.

A CASE OF PROLONGED GESTATION, WITH AUTOPSY OF THE FŒTUS (Rossié, *Amer. Journ. of Obstet.*, January 1886).—In this case labour commenced 317 days after a single coition; but in support of the patient's own statement, the writer adduces the fact that the child was of extraordinary dimensions, measuring 54 cm. in length, and weighing 5300 gms., while ossification of the fetal head bones and of several centres of ossification in the limbs showed a development of the osseous system more advanced than that which corresponds to the usual time of birth.

A CASE OF ARTIFICIAL VESICO-VAGINAL FISTULA FOR THE CURE OF CHRONIC CYSTITIS (Arthur V. Macaw, M.B., Master of the Rotunda Hospital, *The Dublin Journal*, October 1885).—The writer records two very satisfactory cases in which he employed this method of treatment. In the course of the paper he says,—There is one point about the formation of the stone in this case to which I would like to draw attention. When there is a small fistula high up in the vagina, the condition of the bladder seems to me to resemble very closely a cistern of water with an overflow pipe near the top. The urine accumulates in the bladder, and finally reaches the level of the fistula, through which it escapes. The bladder is therefore never excited to contract and expel its contents per urethram, and the urine below the fistula stagnates and deposits its salts, which form a calculus. In a very similar case of vesico-vaginal fistula that has more recently come under my notice, I ventured to say that we should probably find a calculus in the bladder, and my supposition proved to be correct.

ON ATRESIA OF THE TUBES, WITH CONSECUTIVE HYPERTROPHY OF THEIR MUSCULAR WALLS (Kaltenbach, *Centbl. f. Gynäk.*, 43, 1885).—As a result of his examination of the tubes removed from a case of gonorrhœal salpingitis, he concludes that the explanation of the hypertrophy was, that the disease of the mucous membrane had not led to complete atresia, but only to stenosis of the uterine end. In cases where the atresia is complete, or where the secretion is very abundant, there would thus be occasioned dilatation of the tubes, and in such rare cases as the one he reports, eccentric hypertrophy of their walls.

OCCASIONAL PERISCOPE OF THE DISEASES OF CHILDREN.

By CHARLES E. UNDERHILL, M.B., F.R.C.P.E., Physician to the Royal Hospital for Sick Children.

TWO CASES OF EMPYEMA FOLLOWING SCARLET FEVER.—Two moderately severe cases of scarlet fever were followed by albuminuria and anasarca, and when the dropsical condition had nearly disappeared, the fluid began to collect in the pleural cavity. In one case the right side, and in the other the left was affected. The cases were remarkable, chiefly from the quantity of the fluid. In case I., a boy, aged nine years, twenty-eight ounces of pus were removed by aspiration; and when the chest refilled, and an incision was made and a drainage-tube inserted, forty ounces came away. There was a great falling in of the chest wall, and when last seen, a year later, there was still a slight discharge from the fistulous opening; the child was otherwise in good health. In case II., the fluid would not flow through the aspirator needle, and an incision

was immediately made, which let out "between two and three pints of slightly offensive pus." This child was four years of age. He rapidly recovered from the empyema, but before he was dismissed from the hospital he passed through an attack of typhoid fever, from which also he made a good recovery.—I. N. Marshall, M.D., *Archives of Pediatrics*, January 1886.

A CASE OF UNUSUAL MALPOSITION OF THE VISCERA IN A NEW-BORN CHILD.—The third child of a healthy mother; no previous malformations or deformity in the family history. It gave a few feeble cries, and died in a few minutes. On removing the chest wall and opening the abdominal cavity the following condition was found. The whole of the space usually occupied by the left lung, heart, and great vessels was filled with intestines, the upper third being large intestine, the lower two-thirds small intestine. The right side of the chest contained at the apex the thymus gland; below that, the heart and great vessels covered by the pericardium. To the right of these latter and slightly below was the right lung, trilobed, but in a condition of almost complete atelectasis. On raising the intestines on the left side of the thorax the stomach came into view resting on the convex surface of the diaphragm, with one or two coils of large intestine covering its pyloric end. At its cardiac end was the spleen, and behind and below could be seen the left lobe of the liver passing through a large congenital deficiency in the posterior part of the left side of the diaphragm. The opening was oval, being three and a half inches in breadth, and one inch in its antero-posterior diameter. The edge was smooth, the serous surfaces being continuous. Towards the median line of the thorax, and attached by some fibrous tissue to the vertebral column on the left side, was a small bilobed, yellowish mass, which proved to be the undistended left lung. The ductus arteriosus was found patent; the right pulmonary artery could be traced into the right lung, but a fibrous band represented that going to the left lung. The abdominal cavity was occupied by the liver, the remainder of the intestines, and the bladder. The brain was normal.—John Phillips, B.A., M.B., etc., *Archives of Pediatrics*, January 1886.

ON CEREBRAL PARALYSIS IN CHILDREN.—A valuable paper on this subject was published by Professor Strümpell in 1884, which was noticed at the time in this column. In the present contribution, Dr H. Ranke follows up the subject, and gives an account of nine cases which have recently come under his care at Munich. His conclusions agree entirely with those of Strümpell, and are briefly as follows:—The disease is analogous to spinal paralysis, but is quite distinct from it. Of the initial stage nothing accurate could be learned. Three of the cases were supposed to be congenital; two of these were breech cases, and the third was a contracted pelvis; all three were asphyxiated when seen. Six cases were hemiplegic, the remainder monoplegic. In the hemiplegic cases the arm was

usually more affected than the leg. The growth of the affected arm was checked, and it was shorter than the other; the nutrition of the muscles and bones being interfered with, but the subcutaneous fat was not affected. In the six hemiplegias there was marked athetosis of the arm. The paralysis was not in any of the cases of the loose, flabby character seen in poliomyelitis, but there was muscular tension, though no contraction. The tendon reflexes were always present, but rarely exaggerated. Sensibility was not lessened. Facial muscles were not involved. The electrical reactions, which are given very fully, were in strong contrast to those of poliomyelitis, *i.e.*, there was no diminution of reaction to either constant or interrupted currents, and there was no reaction of degeneration. In two of the cases there was marked blunting of the intellectual faculties. The symptoms seem to point with great certainty to a lesion of the motor cortical tract of the brain.—*Jahrb. f. Kinderheilk.*, xxiv. 1, 2.

ON THE THERAPEUTIC VALUE OF PREPARATIONS OF COCA IN CHILDHOOD.—Professor Pott of Halle records some experiments with coca. He used a tincture composed of one part of coca leaves to five parts of absolute alcohol, and an extract internally, and solution of the strength of five and ten per cent. of cocaine for subcutaneous injection and external application. He found the tincture, in doses of from five to twenty drops every hour or two hours, of the greatest benefit in the various forms of enteritis, gastro-enteritis, colitis, and cholera nostras, in children under two years of age. Even in very severe and desperate cases, after from 50 to 100 drops of the tincture had been taken, *i.e.*, within about twenty-four hours, the sickness stopped, the diarrhoea was greatly lessened, and a permanent improvement set in. Part of this improvement may have been due to the alcohol, and the ordinary rules for diet and hygiene were at the same time rigidly enforced. He found no benefit from its use in diseases of the respiratory tract or of the nervous system. The extract was not fully tried, but seemed of some use in the nervous attacks of older children. The chief novelty in his use of the cocaine solution was in its application in whooping-cough. He found that painting a 5 per cent. solution on the back of the throat three times a-day had an immediate and strikingly beneficial effect on the paroxysms, reducing them at once in number from twenty or more during the twenty-four hours to three or four, and is of opinion that the course of the disease may be shortened by some weeks by this method of treatment. He found the same application very useful in cases of inflammation of the tonsils and pharynx, and in various forms of angina.—*Jahrbuch. f. Kinderheilk.*, xxiv. 1, 2, 1886.

VARICELLA AND ITS DIAGNOSIS.—Varicella is almost exclusively a disease of childhood—98 per cent. of all cases being (according to Baader of Basle) under 10 years of age. It can be carried by

nurses and others, as well as by simple contact. The incubation, when inoculated by Steiner, was eight days; when contracted in the usual way, it is generally fourteen days, or even a day or two more. The premonitory fever is short, generally less than twenty-four hours, thus showing a marked contrast to varioloid. The eruption is often the first symptom to call attention to the disease, fresh crops appearing each day for four or five days, while the earliest spots quickly become vesicular, dry up, and the scab falls off. As rare complications, there have recently been observed nephritis occurring about a week after the attack, and deep ulceration at the seat of the vesicles, sometimes becoming gangrenous. The diagnosis, when difficult at all, may be very difficult. From syphilitic eruption it is distinguished by the history of the case, the distribution of the vesicles, and the temperature. From varioloid it is to be distinguished by a consideration of all the facts of the case, including incubation, premonitory symptoms, premonitory fever, appearance of the rash, and the character of the temperature—any single symptom may mislead.—Henry Ashby, M.D., *Archives of Pediatrics*, February 1886.

THE RESULTS OF TRACHEOTOMY WITH SYSTEMATIC VENTILATION.
—The results obtained by Professor Ranke are so striking, that his system deserves attentive consideration. Out of 54 cases, in which the operation was performed either by himself or an assistant, 34 (63 per cent.) recovered, and 20 died. In 9 of the cases there was no pharyngeal diphtheria, and 8 of them recovered; in the remaining 45 cases of undoubted diphtheria, 26 recovered, and 19 died. All the cases were under 9 years of age; 4 of them, of which 2 recovered, were under 2 years. The main point of the special system pursued was the use of two rooms, in one of which the door and window were kept constantly open when not in use, and the patients were taken from the one to the other at least twice a day. Before the patients were transferred to the freshly ventilated room, it was warmed up to the necessary temperature. The only local treatment adopted in all cases of diphtheria was frequent gargling, or, if the child were unable to gargle, frequent swabbing with a 3 per cent. solution of carbolic acid. No drugs were administered. To carry out this system thoroughly, a small house with three rooms was rented, which he called a "croup station," and most of the cases were treated there, many of them being transferred there after the operation had been performed in hospital or in private houses. The moving was attended with no bad results, even in winter. It is worthy of note that after the ventilating method here described was set agoing, none of the nurses or attendants caught the disease, though previously such infection not unfrequently occurred. The right time for operation he lays down thus,—“When a child is hoarse, and has lost its voice, has had one or several, even if

short, attacks of dyspnoea, and when the respiration, even in the intervals between the attacks, displays the well-known tone of stenosis of the larynx, then the time for tracheotomy has come." Many of the cases were, however, brought in at a much later stage, and operated on. Full details are given of the method of the operation pursued and the very careful after-treatment, in which there is nothing specially new, and the paper concludes with a tabular record of all the cases, and some remarks upon each. It is well worth careful study.—*Jahrbuch f. Kinderheilk.*, xxiv. 3, 1886.

Part Fifth.

MEDICAL NEWS.

UNIVERSITY OF EDINBURGH.—MEDICAL DEGREES.—The following is the official list of candidates who passed the First Professional Examination in April 1886:—J. H. Bayley, H. R. Bellamy, W. H. H. Bennett, J. J. Beveridge, M.A.; W. T. Blackledge, T. P. Blades, R. L. Booth, J. A. Bower, William Bower, E. W. R. Branch, T. L. Brander, Reginald Broadbent, Barend Burger, Thomas Burns, Robert Cattley, Albert Coleman, M. W. W. Cowan, Peter Crerar, Rhys Davies, Walter Dickson, D. S. Dixon, J. A. Forrest, W. A. Fowler, W. J. Greenham, Alfred Griffiths, A. H. Hallen, R. E. Howell, W. H. Hughes, Walter Hume, David Huskie, M.A.; Jas. Huskie, H. M. Inglis, B.A.; Hugh Jameson, W. E. Jennings, J. L. Jones, A. B. Kenworthy, J. M. Laughton, A. S. Lawrence, O. R. Lewis, J. S. S. Lumsden, Robert Macfarlane, J. T. M'Kay, W. R. Main, Frank Mason (with distinction), F. A. Maynard, George Melville, G. S. Mill, Vincent Milner, J. L. Morrison, J. M. Morrison, J. P. Morton, Alexander Nasmyth, Andrew Newall, Athelstane Nobbs, A. J. Prentice, Robert Renton, C. G. Robson-Scott, R. R. Ross, J. C. Rossie, W. V. Sinclair, G. B. D. Smart, A. E. Taylor, C. T. Te Water, D. F. D. Turner, Gilles Vandewall, J. J. Vernon, G. A. Waith, Charles Waterston, A. R. A. Wilhelm, John Wilkinson, Arthur Wilson, G. C. Winchester, R. E. B. Yelf, C. W. Yeoman.

UNIVERSITY OF EDINBURGH.—The following is an official list of candidates who have passed the Second Professional Examination during April:—J. H. Acheson, William Allan, D. H. Anderson, Mihangel Ap Iwan, I. J. André, L. E. Barnett (with distinction), G. H. Bartlett, G. T. Beaton, C. H. Bedford (with distinction), W. L. Bell, Arthur Bentham, J. T. Borthwick, Edward Brooks, C. W. Brown, A. A. Bruère (with distinction), E. J. Burnett, M. B. Burton, T. W. Butcher, Edmund Capper, G. A. Casalis, W. W. Chamberlain, Joshua Chaytor-White, Douglas Crawford, A. J. W. Dalzell, W. R. Davidson, Walter Denby, H. B. Denham, Duncan Drummond, C. W. Duggan, W. E. L. Elliot, R. H. J. Fetherston, A. L. Gillespie, S. E. Godfray, John Grant, D. A. Gray, George Hardyman, F. J. Hart, Charles Harvey, J. G. Havelock, John Hewat, William Hewat, J. R. Holmes, P. E. el Howie, J. B. Jameson, E. J. Jennings, W. E. Jennings, J. H. Jones, G. E. Keith, Wm. Kemp, Alex. Kirkwood, J. A. C. Kynoch, Wm. Langwill, J. T. C. Leishman, W. H. Lewis, Frank Liddell, A. G. S. Logie, Thomas Luson, J. M. Lyon, D. H. M'Closky, G. O. C. Mackness (with distinction), D. M. MacIachlan, Alexander Macleay, James Macleod, David Macnish, Christopher Martin (with distinction), Alan Matheson, James Moir, F. H. Morison, R. H. Mortlock, James Mowat, C. D. Musgrove, M. F. X. Nalletamby, J. T. C. Nash, W. S. Nason, G. F. Odhams, William Parker, C. S. Patterson, J. A. Perez, F. U. Purchas, Charles Ramage, G. A. O. Reid, G. F. Rhodes, Walter Rigby, R. F. Robertson, M. A. (with distinction), W. G. A. Robertson, C. G. Savary, Walter Scott, W. C. Sillar, J. B. Simpson, M.A.; G. T. Smith, W. J. Smyth, J. W. Somerville, John Somerville, W. C. Sprague (with distinction), W. H. S. Stalkart, C. B. Stiles, G. W. Sutherland, Joao Teixeira, C. L. Terry, Evan Thomas, G. R. Thomson, E. B. Turner,

T. H. Underhill, George Wade, Thomas Walcott, Q. M. Wallace (with distinction), J. W. Wells, P. J. White, Thomas Whitelaw, H. T. Wickham, A. H. Williams, J. C. Williams, W. W. Williamson, James Wilson, Robert Wise, Percy Wisewould, D. J. Wood, Walter Wynne, Andrew Young.

ROYAL COLLEGES OF PHYSICIANS AND SURGEONS, EDINBURGH.—The following gentlemen passed their final examination for the double qualification in Medicine and Surgery at the sittings held in April and May 1886, and were admitted L.R.C.P. and S. Ed.:—David Denholm Black, Cockburnspath; Frederick Buckham, Hereford; John Connell Knock, Rochestown, Co. Cork; Alfred Owen Davies, Barmouth; Henry Caudwell, Wallingford, Berks; Sidney Edward Percy Cade, Cork; Thomas Valentine Devey, Wolsingham, Darlington; Jonathan Clarke, Cork; William Henry Carter, Aberfell; Devereux Gwynne Hughes, Llandilo; Henry Benson Goulding, Dublin; James Angus Haggart, Edinburgh; David Hume, Edinburgh; Thomas Joseph Jones, Rotherham; William John Morton, Edinburgh; Edwin Priestly, Saintfield, Co. Down; James Williamson Patrick, Derrylane, Belturbet, Co. Cavan; Alexander William Mackenzie, Musselburgh; James M'Gaw, Castlewellan, Co. Down; Charles William Reilly, Roscrea; Alexander Smith, Blackrock, Co. Cork; Alexander Rajagopal, Edinburgh; Henry Powell, Dublin; John Joseph Tuohy, St. Just, Penzance; William Shaw, Islandmagee, Co. Antrim; Henry Hugh Williams, Holyhead; William James Van der Vyver, Edinburgh; Charles Wicks, Newcastle-on-Tyne; Robert John Willis, Belfast.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—The following gentlemen passed their final examination at the sittings held on 8th May 1886, and were duly admitted L.R.C.P. Ed.:—Arthur Somers Bradford, Chippenham, Wilts; Henry Wentworth Windsor-Aubrey, Hull; William Duncan, Frome; Robert Henry Currey, Liverpool.

ROYAL COLLEGES OF PHYSICIANS AND SURGEONS, EDINBURGH, AND FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.—The following gentlemen passed their final examination for the triple qualification in Medicine and Surgery at the sittings held in April and May 1886, and were admitted L.R.C.P. and S. Ed. and L.F.P. and S. Glasgow:—Horace Kelly Young, Canterbury; Arnold Edward Thorpe, Northampton; John Wingfield, Edinburgh; William Edward Bennett, Leeds; George Robert Nelson, Glasgow; William Colquhoun, Bowling; Edward Armitage, Ben Rhydding, Yorkshire; Geo. Alex. Armstrong, Victoria, Australia; Alfred Duckworth, Accrington; Charles Edward Dew, Weston-Super-Mare; James Gordon, Annalong, Co. Down; Adelbert Hanna, M.D., Edinburgh; Robert Nelson Horton, London; James Gardner Holmes, Leeds; James Lychlander, Edinburgh; John Lyon, Glasgow; William Murray Mackay, Eshe, Durham; Sydney Rumbold, Edinburgh; Percy John Wilkinson, Manchester; Alfred Henry Whittell, Leeds.

ROYAL COLLEGE OF SURGEONS, EDINBURGH.—The following gentlemen passed their final examination at the sittings held in April and May 1886, and were admitted L.R.C.S. Ed.:—James M'Nish, Walsall; Wm. Henry Clarke, Macclesfield; James Sinclair Tait, Newfoundland. And the following gentlemen were admitted L.D.S. Ed.:—Edward Percy Rose, Edinburgh; John Trude Frupp, Willemsen.

UNIVERSITY OF GLASGOW.—The following gentlemen have passed in the under-noted Examinations during April 1886:—*First Professional Examination.*—John B. M. Anderson, Gilbert A. Bannatyne, Thomas Berry, W. M. Boyd, Robert Calderwood, Andrew Copland, John Crawford, Andrew Davidson, M.A.; Duncan Davie, John P. Dunn, Charles J. Fyfe, Peter Gardiner, Harry W. Gentles, James Gillespie, George Gordon, Richard Hamilton, Charles A. Henderson, John M. Johnston, Robert B. Lothian, George Mathieson, Duncan M'Callum, Duncan M'Intosh, Robt. D. Mackintosh, Charles N. M'Quarie, John W. Nichol, David Ramsay, William Robb, John Rowan, John Sandilands, M.A.; John Stewart, Paul Stewart, Robert Stobo, James Wallace, Robert Weir, Martin Whyte, Walter E. Williams. *Second Professional Examination.*—Robert Banks, Matthew Beattie, M. A. Boyle, Alexander Campbell, John Crawford, James B. Dalrymple, Thomas J. Davies, Fred. L. Duncan, Frederick Fedarb, Peter Ferguson, R. J. Freebairn, Hugh Girvan, Archibald Gunn, Charles H. Hallet, Thomas W. Hay, William R. Jack, Arthur H. Jago, Thos. W. Jenkins, M.A.; Thomas Laird, George Lapraik, John Melville, J. T. C. Mitter, J. F. D. Macara, John Macarthur, James M'I. M'Call, Duncan M'Eachern, John K. Mackinnon, W. H. M'Kinstry, Hugh Park, J. S. Rankin, Hugh R. Sloan,

William Stewart, John P. Tannock, John Vallance, James D. Wilson, James P. Wilson, Richard J. Wylie, James Young, Robert Zuill. *Third Professional Examination*:—James Adam, Frederick Ashurst, John Baird, Alexander Butler, Daniel Craig, Alexander D. Crawford, William J. Daly, John Dewar, William Dinsmore, Fred. Duncan, John Edgar, M.A.; A. G. Faulds, Robert T. Ferguson, Henry W. Finlayson, William J. Giblin, John D. Gordon, Andrew Gray, Harry Gray, Robert Greenhill, William T. Hannah, George F. Hilliard, Robert Hogg, George J. Imrie, David Jones, William Livingstone, John Love, J. R. Marshall, George Miller, J. K. Morton, Donald Munro, William H. Murray, John M'Corkindale, Thomas L. M'Farlane, Thomas M'Geoch, Malcolm A. Mackintosh, Alexander H. M'Lean, William MacLennan, Roderick A. Macleod, John MacPherson, William F. Ness, M.A.; Thomas J. Redhead, Robert G. Reid, Hugh Rhodes, Andrew H. Richmond, Archibald Roxburgh, Gavin Russell, William Russell, Charles E. Scanlan, Robert Stevenson, J. Mitchell Stewart, David Stone, Andrew Tannahill, E. B. Tant, James A. Ure, William Walker, Dugald C. Watt, Randolph O. Willis, Andrew S. Wotherspoon, John M. Young. The following have also passed in Pathology:—Alexander D. Crawford, Robert T. Ferguson, Andrew Gray, Robert Greenhill, Robert Hogg, David Jones, William Livingstone, George Miller, William F. Ness, M.A.; Archibald Roxburgh, Charles E. Scanlan. W. F. Phillips has passed the First and Second Examinations (old regulations).

MARSHALL'S SEMOLINA.—Among the delicate, often dangerous duties of an editor, perhaps there is none more delicate and dangerous than the experimental researches which have to be conducted in his family circle or on his unsuspecting guests, the testing the nutritive or intoxicating powers of the various samples of food and wine laid on the editorial table. Chemists and microscopists may make their reports, but the real test of the pudding is the preening of it. Marshall's Semolina has stood the test, and by cook and victims has been pronounced excellent.

OBITUARY.

DR WILLIAM CUMMING, F.R.C.P.

Eheu, fugaces, Posthume, Posthume, labuntur anni, and as the years fleet past, the ranks of the profession change in a way that brings a quick sadness to the seniors as they see their friends drop in the race. By the death of Dr William Cumming many of us feel a stage older; for he was about the last of that generation. He was born in 1812. His father was a classical teacher, whose reputation and learning were of so high a standard that he nearly got the Greek Chair in the Edinburgh University when Dunbar was the successful candidate. He gave his sons, including William, a classical education so full and precise that father and sons were able to converse regularly in the Latin language. This classical learning our friend kept up so well that to the last he was able to read both Latin and Greek fluently, and did so not as a task, but for pleasure.

He was first intended for the Church, and had passed all the required examinations, but had always had a love for the

physical sciences, especially for medicine, and this natural bent of his was changed into a determination to study it almost accidentally. He was taking a walk in the country with his brother James, when they called in at Newington House, then inhabited by George Bell, who, during conversation, discovered William Cumming's desire to be a doctor, kindly encouraged him, took him as an apprentice to the firm of Bell, Russell, & Co. without a premium, and thus settled the change in his plans. During the time of uncertainty, William Cumming consulted his minister, the Reverend Robert Gordon, afterwards of the Free High Church, who, seeing the bent of his mind, encouraged him in his decision for medicine. In his apprenticeship in St Andrew Square he was well trained by the Bells—George, Joseph, and Benjamin—and was a fellow-apprentice of his old friend Dr T. H. Pattison, and other well-known men. He graduated at St Andrews, and took the L.R.C.S. in 1839, and soon afterwards settled in practice in Edinburgh,—first living in a small house in Elder Street. With few friends, no great connexion, and no money, he fought an uphill battle; assisted his practice by private teaching, at which his great classical knowledge made him successful. He started a private dispensary, which taught him much. He worked at electricity, at diseases of women, at pathology. He attracted the notice and gained the friendship of Abercrombie, and was one of the few present at the post-mortem examination of that great physician. Once Cumming got his foot on the first rung of the ladder his progress was sure, and soon rapid, for he had the great gift of securing the absolute confidence and affection of his patients; so for many years he worked hard at a private practice both large and lucrative. His sound sense, his impressive manner, and, above all, his constant and attentive kindness, were characteristic of the model family physician, and in some departments his special knowledge and experience gave him the position of a consultant.

For a long time Dr Cumming enjoyed excellent health, looked younger than his years, and gave promise of a long, green old age, but the death of a beloved and promising son, which was soon followed by the death of his wife of forty years, was a great shock to him. Several attacks of heart illness made it obvious to all his friends that his working days were nearly over. Still, he wished to work a little, and his advice was much prized. He tried to visit a patient or two, and on 27th of April, after a short drive, he died in his chair without a struggle.

Cultured, well read, and experienced as Cumming was, his devotion to practice interfered with literary work, so his contributions to the literature of his profession are few. He was much interested in his own College of Physicians, was for many years an examiner, and it is an open secret that had his life been spared the highest honour the College can confer would have been offered to him.

He has left an only surviving son, a graduate of Edinburgh, who succeeds him in his practice.

THE LATE DR SCOTT ORR OF GLASGOW.

(Condensed and corrected from the *Glasgow Herald*.)

No one could possibly be less disposed than Dr Scott Orr to court notoriety, or even to assume undue prominence in those public positions to which he was called by the opinion of his professional brethren, his ideal of life having been throughout that of the trusted and confidential adviser of families, rather than that of the public teacher or public functionary, and his conduct always in strict conformity with this ideal. But as a bright example of a pure and noble career, in a profession in which such examples, it is true, are by no means rare, and as a striking illustration of one suddenly called away from the post of duty—so suddenly that it might almost be said that no note or symptom of suffering had given warning of the event—his life and death may fitly become the subject of a brief notice in these columns. Dr Scott Orr was walking home along Sauchiehall Street on Saturday last at the close of a day's work; no one of his numerous friends knew that he was at all indisposed, nor would any one in all probability have become aware of the fact unless on account of his entering a chemist's shop by the way, and causing a simple remedy for some dyspeptic trouble from which he suffered to be made up on the spot. He reached home, sat down in his arm chair, and seemed to be better, or, indeed, well again; when, in a moment, without an audible expression of suffering or of the sense of danger, without even a change of position from the perfectly unconstrained one in which he had been seated, he was observed to be breathing rapidly and in an unusual fashion, and in a few seconds more was dead! No rhetoric can point the moral of such an event as this more strongly than the bare statement of the facts; and no friend of the deceased physician, but must feel in his inmost heart that when a good man is thus taken away it is well with him. *Felix opportunitate mortis*, he has been saved all the pain, the struggle, the anguish of parting; he leaves to his family the example of an active life worthily sustained to the end, and the tradition of a character not only unblemished, but singularly devoid even of the semblance of reproach, and deeply planted in the affections of old and trusted friends—for he had no enemies, and his friends were the friends of a lifetime.

Dr Scott Orr was born at Edinburgh in 1819, and had therefore attained and passed, but only by a few weeks, his 67th year. He was educated at the High School, and afterwards at the University

of Edinburgh, where he took the degree of M.D. in 1840, and in the same year became a licentiate of the Royal College of Surgeons, of which, also, at a later period, he became a Fellow. His earliest experience of a medical life was gained as assistant to the late Dr Hutchison of Gartnavel. In 1844 he became medical superintendent of the Glasgow Royal Infirmary, and here he not only became familiar with hospital administration in quiet times, but had to bear a large share in the responsibility of the special provision for the great epidemic of fever in 1847, so well remembered, even now, by those who can include it in their experience, as one of unexampled severity. After this Dr Orr settled in private practice in Dunoon, where he remained for seven or eight years, afterwards returning to Glasgow to occupy a house in Bath Street, and at a later period in Albany Place, and to assume, what he never abandoned to the latest hour of his life, the habits and ways of the family physician among us. As compared with this field of labour all his other duties were merely episodes and adjuncts. For many years, however, probably for a longer time than any member of the profession now living, he acted as a physician to the Royal Infirmary; and every one who knew him in that capacity knew also that he was devoted to and proud of his work. He became a Fellow of the Faculty of Physicians and Surgeons in 1856, and was sent to represent the Faculty in the General Medical Council on the death of the late Dr J. G. Fleming in 1879. This office he held until his death, and during his incumbency he also occupied for three years the position of President of the Faculty, thus proving in a twofold sense the confidence placed in his judgment by his fellow-practitioners in Glasgow and the neighbourhood. In the Medical Council we have reason to know that his opinions, though not dogmatically or too strongly expressed, were always regarded as those of one having the interests of the whole profession and of the public thoroughly at heart; and his essentially friendly and sociable nature found great relief and enjoyment in the companionships which his repeated visits to London afforded him. He also acted for many years as Examiner in Medicine in the University of Glasgow, as well as in the Faculty of Physicians and Surgeons, and in all the many differences that have arisen between the Universities and the Corporations on the vexed questions of medical reform, he never once lost the entire respect, and often more than respect, of his colleagues in either of these institutions. These, however, were the only public functions that he fulfilled, or perhaps even so much as aimed at fulfilling, if we except the duty of giving lectures on clinical medicine, which came to him as a part of his official work as a physician to the Royal Infirmary. It was as a private practitioner or family physician that he chiefly desired to be regarded, and his success in this respect was well earned, and, so far as we know, never gave rise to the smallest jealousy or

misunderstanding among his brethren. He was most punctual in the discharge of duty, and yet not exacting as regards others; altogether devoid of self-assertion and of the morbid sensitiveness which usually goes along with it; well-informed in his vocation, without being pedantic or forward to display learning; and always guided, both in word and deed, by the highest standards of professional conduct. The writer of this notice has had the most ample reason to know every detail of his behaviour throughout several seasons of attendance involving weeks of extreme anxiety, and can bear witness that at no moment was he other than the same sympathizing friend and trusted adviser as he was on the most ordinary or trivial occasions. But the experience of one must be multiplied by hundreds or thousands in order to give the sum total of what is due to the modest services of a man like Dr Orr. Surely "the blessing of them that are ready to perish" has often been his, and the sorrowing but heartfelt awe that has attended his sudden removal from among us will not be soon forgotten.

PUBLICATIONS RECEIVED.

- A System of Practical Medicine by American Authors. Vol. IV. Sampson Low & Co., Lond., 1886.
- JAMES H. AVELING, M.D.,—Inversion of the Uterus. J. & A. Churchill, Lond., 1886.
- A. J. BERNAYS, Ph.D., etc.,—Notes on Analytical Chemistry for Students in Chemistry. J. & A. Churchill, Lond., 1886.
- J. MITCHELL BRUCE, M.A., M.D.,—Materia Medica and Therapeutics. Cassell & Co., Lond., 1886.
- HORACE DOBELL, M.D.,—Asthma: its Nature and Treatment. Smith, Elder, & Co., Lond., 1886.
- GEO. FLETCHER, M.D.,—The Management of Athletics in Public Schools. H. K. Lewis, Lond., 1886.
- RICHARD GIBBS, Registrar-General of Victoria,—Patents and Patentees. Melbourne, 1885.
- G. W. HAMBLETON,—What is Consumption? J. & A. Churchill, Lond., 1886.
- DR GEORGE HARLEY, F.R.S.,—Inflammations of the Liver. J. & A. Churchill, Lond., 1886.
- CHRISTOPHER HEATH, F.R.C.S.,—Dictionary of Practical Surgery. 2 vols. Smith, Elder, & Co., Lond., 1886.
- E. KLEIN, M.D., F.R.S.,—Micro-Organisms and Disease. Macmillan & Co., Lond., 1886.
- JOHN MACPHERSON, M.D.,—Bath, Contrex-éville, and the Lime Sulphated Waters. H. K. Lewis, Lond., 1886.
- WM. MARTINDALE, F.C.S.,—Coca, Cocaine and its Salts. H. K. Lewis, Lond., 1886.
- CHARLES D. F. PHILLIPS, M.D.,—Materia Medica and Therapeutics. J. & A. Churchill, Lond., 1886.
- H. SAXON SNELL,—St Marylebone Infirmary. B. T. Batsford, Lond., 1886.
- WM. SQUIRE, M.D.,—On Glycosuria. Lond., 1886.
- LEWIS A. STIMSON, B.A., M.D.,—Manual of Operative Surgery. H. K. Lewis, Lond., 1886.
- C. S. TAYLOR, M.D.,—The Alkaline and Non-Alkaline Treatment of Acute Rheumatism. Henry Kimpton, Lond., 1885.
- Transactions of the American Gynecological Society. Vol. X. D. Appleton & Co., New York, 1886.
- Transactions of the Obstetrical Society of London. Vol. XXVII. Longmans, Green, & Co., Lond., 1886.
- Transactions of the Obstetrical Society of London. Vol. XXVIII. Part I. Lond., 1886.
- W. H. WALSH, M.D.,—The Colloquial Faculty for Languages. J. & A. Churchill, Lond., 1886.
- E. J. WARING, C.I.E., M.D.,—A Manual of Practical Therapeutics. J. & A. Churchill, Lond., 1886.
- Year-Book of Scientific and Learned Societies of Great Britain and Ireland. Charles Griffin & Co., Lond., 1886.
- ZIEGLER'S Handbook of General Therapeutics. Vol. V. Smith, Elder, & Co., Lond., 1886.

PERIODICALS RECEIVED.

- Abstract and New Books,—Feb.
 Albany Medical Annals,—Oct.—March.
 Alienist and Neurologist,—Oct.—Jan.
 American Druggist,—Nov.—Dec.
 American Journal of Obstetrics,—Nov.—April.
 American Journal of the Medical Sciences,—Oct.—April.
 American Lancet,—Jan.—May.
 American Practitioner,—Oct.—Feb.
 American Practitioner and News,—Jan.—May.
 Annales d'Oculistique,—Sept.—Feb.
 Annales de Dermatologie et de Syphilographie,—Oct.—April.
 Annales des Maladies de l'Oreille, etc.—Nov.—April.
 Annales Médico-Chirurgicales,—Nov.—April.
 Archives de Tocologie,—Nov.—April.
 Archives Générales de Médecine,—Nov.—May.
 Archives of Gynecology,—Feb.—April.
 Archives of Pediatrics,—Oct.—April.
 Archivio di Ortopedia,—Dec.
 Aselepiad,—Nos. 9, 10.
 Australasian Medical Gazette,—Sept.—Mar.
 Berliner Klin. Wochenschrift,—Oct. 26—May 17.
 Birmingham Medical Review,—Nov.—May.
 Births, Deaths, and Marriages, Monthly Return of,—Oct.—April.
 Do., Quarterly Return,—March.
 Do., Supplement.
 Boston Medical and Surgical Journal,—Oct. 15—April 29.
 Brain,—Oct.—April.
 Bristol Medico-Chirurgical Journal,—Dec.—March.
 British and Colonial Druggist,—Jan.
 British Gynecological Journal,—Nov.—Feb.
 British Journal of Photography,—Jan.
 British Medical Journal,—Oct. 31—May 29.
 Bulletin Général de Thérapeutique,—Oct.—May.
 Centralblatt für Gynäkologie,—Oct. 31—May 15.
 Centralblatt für Chirurgie,—Oct. 31—May 15.
 Chemist and Druggist,—April 24—May 15.
 Chicago Journal,—Oct.—April.
 Christian Treasury,—Jan.
 Detroit Lancet,—Nov.—Dec.
 Dietetic Reformer,—March.
 Dublin Journal of Medical Science,—Oct.—May.
 Ephemeris of Materia Medica,—Nov.
 Fortschritte der Medicin,—Dec.
 France Médicale,—Oct. 27—May 15.
 Gazette de Gynécologie,—Nov.—May.
 Gazette des Hôpitaux,—Oct. 27—May 15.
 Gazette Médicale de Paris,—Oct. 31—May 15.
 Gazzetta Medica di Torino,—Oct. 15—April 5.
 Glasgow Medical Journal,—Nov.—May.
 Indian Medical Gazette,—Jan.—March.
 International Journal of the Medical Sciences,—Jan.—April.
 Jahrbuch für Kinderheilkunde,—Band 24, Heft. 1-4.
 Journal de Médecine de Bordeaux,—Oct. 25—May 16.
 Journal de Médecine de Paris,—Nov. 1—May 16.
 Journal of Anatomy and Physiology,—Oct.—April.
 Journal of Cutaneous and Venereal Diseases,—Nov.—March.
 Journal of Mental Science,—Jan.—April.
 Journal of Nervous and Mental Diseases,—Oct.—March.
 Journal of the American Medical Association,—Jan. 2—May 1.
 Journal of the British Dental Association,—Nov.—March.
 Klinische Monatsblätter für Augenheilkunde,—Nov.—May.
 Liverpool Medico-Chirurgical Journal,—Jan.
 London Medical Record,—Nov.—May.
 Medical Chronicle,—Nov.—May.
 Medical Press and Circular,—Oct. 28—May 5.
 Medical Reformer,—Jan.—April.
 Medical Times and Gazette,—Oct. 31—Dec. 26.
 Medicina Contemporanea,—Nov.—Feb.
 Medico-Legal Journal,—March.
 Monitor Medico,—Sept.—Nov.
 Nashville Journal of Medicine and Surgery,—Feb.
 New Orleans Medical Surgical Journal,—March.
 New York Medical Journal,—Oct. 17—May 1.
 New York Medical Library,—March.
 New York Medical Record,—Oct. 17—May 1.
 New York Medizinische Presse,—Dec.
 Nordiskt Medicinskt Arkiv,—Band xvii. Nos. 9-31.
 Normandie Médicale,—Nov.—Dec.
 Nouvelles Archives d'Obstétrique et de Gynécologie,—Jan.—April.
 Once a Month,—Sept.
 Pharmacist,—July—Dec.
 Philadelphia Medical and Surgical Reporter,—Oct. 17—May 1.
 Philadelphia Medical News,—Oct. 17—May 1.
 Philadelphia Medical Times,—Oct. 17—May 1.
 Polyclinic,—Oct. 15—April.
 Practitioner,—Nov.—May.
 Proceedings of the Society for the Study and Cure of Inebriety,—Nov.—May.
 Progrès Médical,—Oct. 24—May 15.
 Provincial Medical Journal,—Dec.
 Rassegna di Scienze Mediche.—Jan., Feb.
 Recueil d'Ophthalmologie,—Oct.—Dec.
 Revista Argentina de Ciencias Médicas,—April—Dec.
 Revue de Chirurgie,—Nov.—May.
 Revue de Thérapeutique Medico-Chirurgicale,—Nov. 1—May 15.
 Revue Médicale,—Oct. 31—April 3.
 St Louis Courier of Medicine,—Nov.—May.
 Therapeutic Gazette,—Oct.—April.
 Transactions of the Sei i Kwai,—Sept.—Mar.
 Virchow's Archiv,—Nov.—April.
 Weekblad van het Nederlandsch Tijdschrift voor Geneeskunde,—Oct. 17—May 15.
 Western Medical Reporter,—Nov.—April.

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- ALLBUTT, T. CLIFFORD, M.D., on scrofulous neck, *rev.*, 264.
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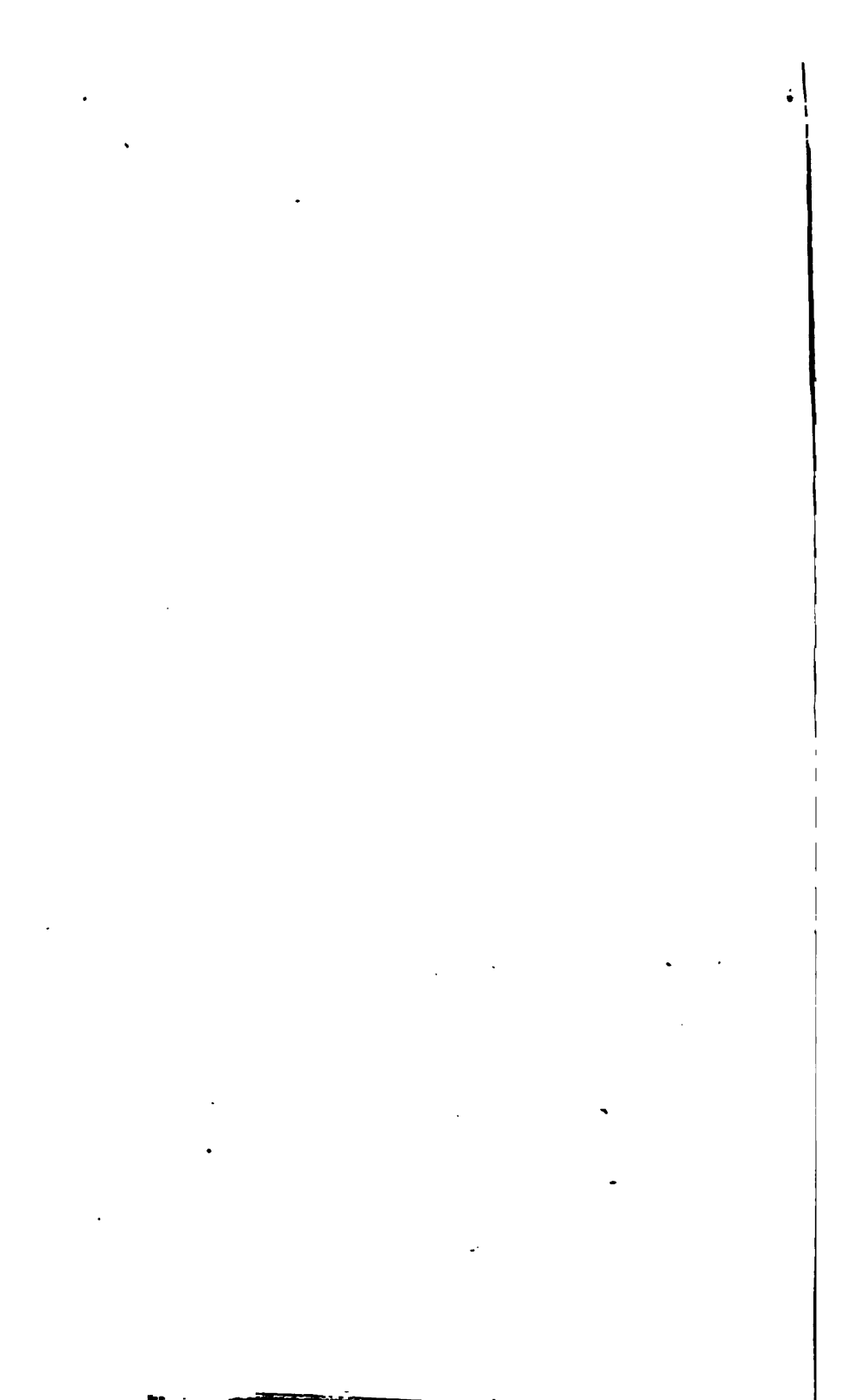
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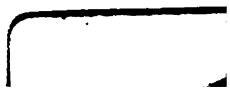
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